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PLAGIOCLASE TWINNING

BY R. C. EMMONS AND R. M. GATES

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ABSTRACT

This is an attempt to interpret some of the various aspects of plagioclase twinning and to use twinning as a clue to the environmental conditions at the time of crystallization.

INTRODUCTION AND SUMMARY

The purpose of this paper is (1) to fix the age of polysynthetic twinning of plagioclase feldspar with respect to the age of the crystal; and (2) to use the nature of the twinning to gain an insight into some of the conditions which prevailed at the time of crystallization. We believe that even a cursory study of only the general twin pattern in a rock slide will reveal much information valuable in understanding conditions during crystallization of ordinary feldspar-bearing rocks.

This work was supported in part by a grant from the Wisconsin Alumni Research Foundation.

Our main conclusions are:

(1) In many sections studied the polysynthetic twinning developed quite late in the crystallization period, in fact after almost all feldspathic liquid was gone. We believe that the burden of proof is on those who claim on theoretical grounds that the polysynthetic twinning of plagioclase develops with the crystal. Tentatively we believe that practically all polysynthetic twinning is formed late in the crystal's growth.

(2) A common force leading to polysynthetic twinning is the mutual interference of growing crystals. A phenocryst ordinarily suffers little twinning in a fine-grained groundmass.

(3) Relatively intense stresses break the crystal and cause little twinning. Intermediate stresses lead to fine twinning, and mild stresses to coarse twinning. Twinning is a comparatively slow crystal response to stress.

(4) The stress imparted to a crystal by a viscous liquid moving about it may cause twinning.

(5) One part of a crystal which is selectively stressed may be exclusively or differently twinned.

(6) Persistent twin lamellae signify conditions favorable to twinning. Discontinuous twin lamellae characterize metamorphic and hybrid rocks.

(7) Twinning once developed in a crystal is continued on further growth without apparent modification, except in some late growths such as the thin mantles or welds of fractures. Such mantles or welds are presumably caused by deuteric solutions rather than by the earlier, more nearly "igneous" solutions.

(8) The width of the twin lamellae is determined by several factors but mainly by the environment during late crystallization—that is, by the intensity of the structural disturbances of the rock and crystal and by the rigidity or viscosity of the rock at that time.

(9) There is no observed indication in our study that the normal atomic structure of a crystal, or its composition alone, may determine the nature of the twin lamellae. This is consistent with the common occurrence of coarse twinning in calcic plagioclase, which we attribute to environmental factors.

(10) Adjacent twin lamellae quite commonly differ in composition, especially in the more sodic plagioclases, apparently as a result of selective post-twinning replacement through deuteric solutions. These differences are not limited to anorthite content but may also be expressed by sericitization, kaolinitization, chloritization, and doubtless by many other replacement types. This further limits the period of twin formation—antedating deuteric solutions.

GENERAL CONSIDERATIONS

Although plagioclase feldspars are not always twinned, they are twinned so much of the time that it is customary to assume that an untwinned feldspar is "orthoclase". Either, one ordinarily fails to recognize untwinned plagioclase, or untwinned plagioclase is comparatively uncommon. In any case twinning in plagioclase is taken quite for granted.

The almost total lack of use that we make of plagioclase twins is ample testimony to our failure to understand their significance. One, two, or three of the several twin laws may characterize plagioclase in a given rock, but no sound claim has ever been made to understand or explain the twinning (Chapman, 1936). Similarly, the composition face is much more irregular in some rocks than in others—so much so that Manolescu (1934) has pointed out that the composition face is not the best indicator of plagioclase composition, as used on the Fedorow stereograms (Emmons, 1943). Some twin lamellae are, without explanation, much more persistent than others as illustrated in the accompanying photomicrographs. Calcic plagioclase tends more to twin on (001) as a composition face than does sodic plagioclase; all plagioclase twins most commonly on (010) as composition face.

Donnay (1940) supports this view, rather widely held, that the age of a given twin is the age of the seed crystal. Our study has convinced us that this is true only for the simple twins—Carlsbad, Manebach, and Baveno,—and not for polysynthetic twins. It is proposed, here, to attempt to solve some of these puzzles.

Perhaps, as Donnay suggests, Friedel first placed an age on polysynthetic twins. In Friedel's 1904 edition (p. 184) he says (in translation):

"And, second, [in] polysynthetic twins, at anytime during the growth of the crystal, the material may almost indifferently adopt the normal position or the twin position, in such a way that the twin is repeated frequently in the same structure. The two orientations are then positions of equally possible equilibrium. Uniquely, mechanical twins fall in this category (see calcite)."

This opinion was expressed again by Friedel in his 1911 edition on page 227. In his 1926 edition (p. 440) he says (in translation): "During crystallization when new particles come to join the crystal structure they can adopt almost indifferently either position of equilibrium." Clearly Friedel believes that polysynthetic twins develop with the crystal's growth.

In regard to the development of polysynthetic twins Donnay (1940, p. 579) says:

"In the case of very easy twinning, the prolongation of the twin lattice from one twinned individual to the other is nearly perfect and the particles that concur to the building of the crystalline edifice (be they ions, atoms, molecules) will, at every

moment during growth, be solicited to adopt either one of two orientations, namely that of the original crystal and that of its twinned symmetrical."

Donnay is in complete accord with the French theory of twinning as stated by Friedel.

The mechanical origin of polysynthetic twins has been suggested occasionally though with considerable reserve. In 1931 Phillips (p. 229) observed:

"Two instances of the true pericline twins in pure albite . . . in porphyroblasts in clorite-albite-epidote schists (green beds) from the Dalradian succession of Scotland. . . . It is perhaps worth noting that this association with stress conditions is not unnatural. Müggе has pointed out that the rhombic section as composition plane is to be expected if the twins are produced by simple gliding. . . . Actually the pericline twin assumes special importance in metamorphic schists and gneisses . . . as observed by Becke, by Duparc, and Rheinard, and by myself. The well formed albites of the Binn Valley occurrences are presumably growth twins and nothing is at present known of the factors determining twinning under these conditions."

Ramdohr in 1936 says (p. 136, par. 75, in translation): "The occurrence of numerous fine polysynthetic lamellae is very characteristic of all glide twinning." We believe this to be a very pertinent statement.

Donnay does not overlook other factors which influence twin formation. He says (1940, p. 579): "I am confident that with regard to twinning phenomena especially, the mass of available evidence supports the view that internal factors strongly outweigh external ones." With this we disagree. If internal factors control the width of albite twin lamellae then the twinning forms with the crystal's growth. We shall offer evidence that twinning forms late in the crystal's growth. If this evidence is accepted Donnay's view cannot hold.

TIME OF TWIN FORMATION

We shall attempt to show that in some cases of apparently "normal" crystallization the crystal started to grow free of twinning and became twinned either late in its growth or after it was fully grown. We shall show further that in other details the nature of the twinning is related to the environment which prevailed at or near the close of crystallization.

The age of twinning relative to the period of the crystal's growth is perhaps best determined by studying the relationship of the twin lamellae to the superimposed features of the crystal. Five crystals have been selected to illustrate the trend of this line of evidence. In Figure 4 of Plate 1 the[1] twin lamellae are very definitely related to the welded fracture which cuts the entire crystal. Two lamellae which cross the fracture and are appar-

[1] Some of our illustrations have been taken from the petrographic collection of the University of Wisconsin to supplement our own material. All magnifications are ocular times objective and omit the enlargement in the camera bellows and the reduction in engraving.

ently pre-fracture in age are distorted at the fracture. The other lamallae, being terminated by the fracture, must be post-fracture in age. Most of the lamellae postdate the fracture. But since the fracture cuts the entire crystal the fracture essentially postdates the crystal. Then the twinning must also postdate the crystal. However, since feldspathic material welds the fracture there must have been traces of late magmatic solutions still present. The welding of the fracture, with interrupted lamellae, suggests that twinning preceded late magmatic solutions. We shall show that added feldspar such as this weld is susceptible to twinning and will not inhibit the growth of twin lamellae across it. Evidence of this type is not difficult to find in routine rocks, though it is not often so complete and limiting as in the crystal illustrated.

Figures 2, 3, and 7 of Plate 1 show crystals in which the distortions are marked and are quite evidently late in the growth of the crystal. Figures 3 and 7 show specimens from parts of the Duluth gabbro, having distinct flow structure as an original characteristic of the rock—that is, a noticeable parallelism of feldspars but no schistosity or brecciation such as would be expected of a subsequent disturbance. The twinning in these crystals is again clearly related to the distortion and therefore as late as the distortion; it is therefore superimposed on the fully grown crystal. We believe that the twinning was formed at the time of crystal distortion—very late in the crystallization period of the host rock.

In Figure 1 of Plate 1 the broad interpretation is less clear, though the twinning quite evidently postdates the fracture which cuts the entire crystal. Here, too, the twinning must be later than the crystal's main growth, since the corresponding parts of the crystal on opposite sides of the fracture are not similarly twinned. A subsidiary fracture on the left side of the crystal appears to be later than the twinning. The matrix to this anorthosite phenocryst shows shattering.

These selected crystals are apparently normally twinned plagioclase in a normal rock. There are no untwinned plagioclase crystals in the host rocks in the sections studied, and there is no reason to believe that these selected crystals grew either before or after other plagioclase crystals in the respective rocks. Nor is there any reason to believe that the twinning of these selected crystals in any way differs from the twinning of the associated plagioclase crystals. If the evidence presented, dating the twinning, is accepted as proof that the specific twins described formed late in the crystal's period of growth, then the other twins in other crystals in these rocks must have the same age relationship. And these are ordinary igneous rocks. Commonly, if evidence on the age of twinning is sought, some may be found. Such observations have led us to the conclusion that polysynthetic twinning is a late crystallization feature. This

view is directly opposed to the opinions of Friedel (1926) and, more recently, of Donnay (1940).

In some crystals we may further limit the time at which twinning developed by the mineralization which followed it. A fractured crystal may be welded with twinned feldspar (Pl. 1, fig. 1) or just closely welded after twinning (Pl. 1, fig. 4), or welded with untwinned feldspar (Pl. 1, figs. 5, 6). Crystal enlargements normally continue the twinning already formed (Pl. 3, fig. 3). Sericitic veinlets commonly cut across twinning as do other deuteric minerals. We may conclude that twinning may be early enough to be followed by the crystallization of feldspar which shows twinning, but it is commonly so late that the remaining solutions are essentially deuteric—capable of depositing only the fracture fillings, the thin fringes, or the deuteric minerals in the few remaining openings.

CAUSE OF TWINNING

There are clues to the forces which cause twinning in specific cases. In Figures 2, 3, and 7 of Plate 1, the deformation of the crystal is apparently casually related to the twinning. Not only is the "tapering off" of the lamellae related to the deformation, but the continuity of the lamellae is similarly related, hence the deformation and the twinning are logically cause and effect. The stress was transmitted to the yielding crystal by means of the contiguous crystals which themselves may not have suffered more than similar modification in the process. We shall return to this thought later.

It is not a new concept that stress under the proper environmental conditions may cause twinning in plagioclase. Grout (1932, p. 376) refers to "patchy twinning" as evidence of cataclastic metamorphism. Harker (1932, p. 164) says:

> "In igneous rocks which have sustained considerable stress the plagioclase feldspars are often affected by gliding, the chief direction being that of the albite twinning. The secondary may be easily distinguished from the primary lamellae by their finer scale and inconstant occurrence. They often come in, in evident relation to a slight bending of the crystal, and they are sometimes seen to end abruptly at a crack, which has relieved the strain."

His statement that "the secondary may be easily distinguished from the primary lamellae" would be most difficult to prove and does not agree with our experience. Of greater interest, however, is his expressed opinion that some feldspar twinning is secondary. We suggest that plagioclase twinning *in general*, in the ordinary rocks referred to here, formed toward the close of the period of crystallization as the result of forces external to the crystals. These forces result from several qualifying factors: They may originate in the cooling and associated disturb-

ances of the rock—they are transmitted by the contiguous crystals which may materially modify them—they may even be exclusively attributable to the influence of these contiguous still-growing crystals. This viewpoint regards polysynthetic twinning as a gliding effect and denies that it results from the coincidental attachment of molecules to a growing crystal.

Alling (1936, p. 157) observed that plagioclase phenocrysts, in a felsitic or aphanitic groundmass, are apt to have comparatively few twin lamellae or none at all (Pl. 1, fig. 8; Pl. 2, fig. 1). This is also true of phenocrysts in some vitrophyres (Pl. 2, fig. 5). Still more striking is the limited twinning in very large phenocrysts such as those in the anorthosites of Beaver Bay and Split Rock Point, Minnesota, to which Dr. R. H. B. Jones and the senior writer were kindly guided by Professor G. M. Schwartz of the University of Minnesota. These phenocrysts in extreme cases measure 2 feet long and occur in a coarsely granitic groundmass (Grout and Schwartz, 1939, p. 152). The groundmass plagioclases are well twinned.

Thus, a crystal such as a phenocryst, undisturbed by its surroundings, tends not to be twinned. That is to say small crystals, by themselves, apparently cannot exert sufficient force on a large crystal to cause strain, such as polysynthetic twinning, in it. However, a matrix of small crystals in a crystallizing rock which is suffering movement can cause strain in a phenocryst (Pl. 2, fig 2; Pl. 3, fig. 4). Many examples could be given to suggest that mere vesiculation of a glass may transmit sufficient stress to a suspended phenocryst to cause its twinning (Pl. 2, fig. 8), whereas similarly suspended crystals in a milder environment are free of twinning.

All the illustrations cited are specific cases which, though clear enough in themselves, account for the common plagioclase twin only by extrapolation. Nevertheless, the evidence is strong, positive, and factual. We have found no facts of observation to support the alternative view. We are led to believe therefore (1) that plagioclase is polysynthetically twinned late in the period of the crystal's growth, and (2) that plagioclase does not twin polysynthetically of itself but requires outside influence. If these conclusions are accepted, we may look for the most reasonable external force in a crystallizing igneous rock as the cause of common plagioclase twinning—the mutual interference of growing crystals. We have been unable, however, from thin-section study to relate a given twin development in a crystal to the possible activating crystals around it. In the absence of the third dimension this could be virtually impossible anyhow. For the common case one can do no more than point the accusing finger at this well-recognized, fully competent, and widely prevailing force.

IRREGULAR TWINNING

In our experience there are two distinct types of irregular twinning, and each apparently indicates a different environment at the time of twin formation. The first (Pl. 2, figs. 3, 4, 6) is characterized mainly by relatively narrow lamellae and their lack of continuity through the crystal. The lamellae are numerous and are not uniform in distribution within a crystal. Such twinning is not confined to one rock type only but is associated with rocks or parts of rocks such as the hybrids which have suffered more than usual disturbance. It is also found in zones (Pl. 3, fig. 1) in otherwise normally twinned crystals. In general this type of discontinuous twinning characterizes rocks which have been structurally disturbed but not sufficiently to cause shattering of the twinned crystals. In many gneisses, crystal shattering is normal, accompanied by irregular twinning (Pl. 3, fig. 8). Perhaps this may be interpreted as the inability of the crystal to accommodate the stress either because of the intensity of the movement or because of a dry or otherwise unfavorable environment.

The second type of irregular twinning (Pl. 3, fig. 5) is found in ordinary igneous rocks and may be superimposed on ordinary twinning. Its cause is not apparent; it is sometimes termed strain shadows, or an indication of near shattering. In some sections we have studied this phenomenon is actually twinning. It has been identified by the five-axis method (Emmons and Gates, 1939) as complex twinning according to the albite-Carlsbad law and also according to the more difficultly recognizable laws —Manebach Ala A, and Manebach acline. We regard this as a manifestation of strain just as we do all polysynthetic twinning.

TWINNED CRYSTAL ENLARGEMENT

When a feldspar crystal is once twinned, later growths of feldspar on the same crystal usually continue the lamellation. In Figure 3 of Plate 3 a gabbro feldspar (An_{55}, as determined optically) has been fractured, and an apophysis of granite has filled the fracture. The fractured labradorite crystals have grown from the granitic liquid, the crystal increment being oligoclase (An_{18}). Note that the twin lamellae continue quite without interruption from one plagioclase across the other. The same principle undoubtedly holds in ordinary zoned feldspars. A complication not yet recorded is the pericline composition face in a zoned crystal.

Sometimes untwinned feldspathic material is added, either as a thin fringe or as a fracture filling—a sort of broad weld (Pl. 1, figs. 5, 6). This type of crystal enlargement is most common in the very late stages of crystallization. It appears to be closely associated with the formation

of deuteric minerals, especially with their formation in the associated ferromagnesians. The solutions which produce this enlargement could well be transitional to the deuteric solutions—that is, they are more dilute and lower in temperature. This is consistent with the view expressed next, that mineralizers favor coarser lamellation.

FACTORS MODIFYING TWINNING

The effectiveness of stresses which cause twinning does not depend alone on the intensity of the force or the rapidity of movement. Other factors in the rock, particularly those which modify the viscosity of the remaining interstitial liquid, seem to be quite important.

It is well recognized that *in general* calcic plagioclase and very sodic plagioclase typically have broad lamellae (Pl. 3, fig. 2) and that plagioclase of composition about oligoclase has relatively narrow lamellae. If this differential spacing is due to an inherent crystallographic property, it must be consistent. However, narrow lamellae are not uncommon in calcic and very sodic plagioclase (Pl. 2, figs. 3, 7; Pl. 3, fig. 5), and broad lamellae may readily be found in granites (Pl. 4, figs. 1, 3). A striking refutation of the claim that composition controls the width of twin lamellae is found in Figure 3 of Plate 3, showing the identical lamellae in plagioclase of An_{55} to continue across plagioclase of An_{18} without any apparent change in width. We suggest that crystallizing rocks which yield easily to differential pressures ordinarily have broadly twinned plagioclase and that rocks which yield less easily have typically narrow twin lamellae. In other words the environetal factor is dominant in determining the breadth of the twin lamellae. If we choose the gneisses as a rock type which has behaved viscously under deformation, we find narrow twinning the rule regardless of composition.

We believe the evidence favors an explanation based on the viscosity of the host rock. Grout (1932, p. 195) in his excellent brief summary of magmatic viscosity, recognizes the general opinion in saying: "Magmas may be very highly viscous especially if they are siliceous and not hydrous." Calcic feldspars are not at home in siliceous rocks. The fluidity of those rocks in which calcic feldspars normally occur is well recognized. The low viscosity of grabbros is also evident from the frequency of flow structures in them. Similarly, the very sodic feldspars are usually found in pegmatites—highly hydrous rocks of low viscosity—, and the twin lamellae are again broad. Conversely oligoclase occurs in granites —that is, in siliceous rocks where the water content is much lower than in pegmatites. Donnay (1940) explains coarseness of lamellation on the basis of crystallographic "obliquity". If obliquity were the determining factor as Donnay suggests, there should be more consistency. The rela-

tionship of the width of the lamellae to environment seems to us to be too evident to ignore. We believe that polysynthetic twinning is a relatively slow crystal response to stress, modified or controlled by the environment.

Viscosity and the intensity of the stress alone are not the sole factors. Very intense stress in a rock of even low viscosity will lead either to more complex twinning or to shattering of the crystal (Pl. 3, fig. 4). Many such rocks can be found in basaltic areas. In Figure 7 of Plate 3, a calcic plagioclase shows the effect of less intense stress or lower viscosity. Figure 2 of Plate 3 similarly shows the mildest type of usual twinning in calcic feldspar. Figure 5 of Plate 1, and Figure 1 of Plate 3, show more intense twinning in calcic feldspars—in both there seems good reason to believe the stress was relatively more intense. We do not wish to argue the point that it is or is not the rigidity or viscosity of the host rock rather than the crystal, because a definite answer is not at hand, but such a question is pertinent.

SELECTIVE REPLACEMENT

Adjacent twin lamellae may or may not be identical in anorthite content as indicated by both detailed measurements of their refractive indices (Emmons, 1934) and by careful determination of the orientation relationship between the optical elements α, β, and γ and the crystallographic axes a, b, and c as revealed in Fedorow's stereograms. Since strain and other factors modify the optical properties, there cannot be absolute confidence in any given case where there is apparent composition difference. A careful search over a period of years for feldspar material coarsely enough twinned to make an overall separation of adjacent lamellae possible has been most discouraging. Two such types of plagioclase have been found, and both are unsuitable. They are the very calcic plagioclase, such as the phenocrysts in the porphyritic anorthosites of Minnesota already referred to, and the pegmatite specimens such as the coarsely twinned albites of Amelia Court House, Virginia. In both types the optical properties indicate almost no difference in composition. Composition differences, as indicated optically, are most marked in oligoclase and andesine, where the lamellae are hopelessly narrow.

In lieu of a successful direct attack on the problem an indirect approach has been found which seems to have brought convincing support to the optical data on compositional difference between adjacent lamellae. In Figure 6 of Plate 3, and Figures 2, 3, 4, 5, and 6, of Plate 4, are shown crystals of twinned plagioclase in which alternate lamellae are sericitized or chloritized or otherwise deuterically altered. Crystals of this type

are common in the hybrid roof rocks of the central Wisconsin batholith.

It is most unlikely that the difference in composition of adjacent lamellae illustrated was developed during the twinning process or even immediately after it. More likely, one set of lamellae had a greater susceptibility to replacement than the other and was progressively altered. The indicated alteration is not limited to the anorthite content but doubtless also includes a modification in potash content as suggested by the formation of sericite, and quite evidently includes the introduction sometimes of iron and magnesium as indicated by the selective chloritization. When present detailed studies on feldspar compositions are complete, we hope to be able to throw more light on the possible differences in potash and lime content of adjacent lamellae which show on deuteric alteration.

Selective alteration, similar to that just described, is found in perthite (Pl. 4, figs. 7, 8). Here there is an agreed difference in alkali content, prior to selective sericitization. Possibly such a difference is prerequisite to selective sericitization or chloritization. In any case it is easy to see the analogy between selective sericitization in perthite and in twinned plagioclase, and it is entirely reasonable that the proven alkali difference of the perthites should have its counterpart in the twinned plagioclase. These observations are strong support for the apparent compositional differences in adjacent twin lamellae revealed by optical study.

The principle seems to be well founded that adjacent twin lamellae often have real differences in composition, distinct from the apparent differences attributable to strain. These differences are shown in the perthite alterations to be related to an earlier difference in alkali content. Similar differences in selective alteration in plagioclase may then be similarly interpreted to indicate an earlier difference in alkali content of contiguous twin lamellae. This difference is also indicated by optical data. Therefore, is it not reasonable to accept such indicated conclusions from good optical data in general, and to believe that adjacent twin lamellae do differ in anorthite content when the optical properties suggest that they do?

SIMPLE TWINS

During this study we have observed that though polysynthetic twin lamellae appear to develop at a certain restricted general stage in a crystal's growth in ordinary igneous rocks, Carlsbad twins appear at all stages and in all environments. This would indicate that the Carlsbad twin may start with the seed crystal and grow with it, especially the interpenetration Carlsbad twin. But such is not always the case because Carlsbad twinning is not always confined to two individuals. In the

study of resorbed feldspars by Fries (1939), Mr. Fries called the attention of the senior writer to Carlsbad twins composed of as many as four and five individuals. The occurrence is too rare to offer much opportunity for study, argument, and interpretations, but quite evidently all the Carlsbad lamellae were not born at the same time. In the ordinary case, however, both units of a Carlsbad twin are about equally developed, indicating the twin to be about as old as the crystal. Carlsbad twinning cannot then be used interpretatively as we believe polysynthetic twinning can.

The most challenging aspect remaining to the whole question of polysynthetic feldspar twinning is: What environmental factor or factors in the host rock decide what twin law shall prevail? We mention three observations which may serve as leads but we have no guiding answer to the question.

(1) Coulson (1932) reported that in his experience the comparatively rare Ala B twin law occurs in rocks having feldspar of about An_{33}. If this is found to be *generally* true, composition must be recognized as a prominent factor in the occurrence of at least this twin law.

(2) Chao and Taylor (1940) believe that there are two alternative structures in plagioclase: One characterizes anorthite-rich plagioclase, and the other albite-rich plagioclase. Is it possible that such a difference enters into some kinds of twin lamellation? Could Coulson's observation be explained on this basis?

(3) Acline twinning characterizes the Duluth gabbro as the leading twin having (001) for composition face, whereas pericline twinning characterizes other gabbros which seem to be similar. Since these two laws can frequently be recognized in the field with a hand lens, possibly this lead is one of unusual promise. Gysin (1925, p. 146) in a study of 39 crystals found acline twinning in volcanic rocks and peridine twinning in plutonic rocks.

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Manuscript received by the Secretary of the Society, August 20, 1942.

EXPLANATION OF PLATES

Plate 1.—Selected Plagioclase Twins

Figure

1. This crystal shows twinning which is later than the fracture cutting the crystal. The fracture must be later than the crystal's main growth. The twinning therefore must have developed at least late in the crystal's growth. (Porphyritic anorthosite from the Adirondacks. ×16. Plagioclase An_{50}. Albite twin law.)
2. Distorted plagioclase crystal in which distortion covers width of entire crystal and must therefore postdate the crystal's main growth. Since the twinning is quite apparently related to the distortion, the twinning is also dated late in the crystal's growth period. (Gabbro from Schliesen, Germany. ×16. An_{75}. Albite twin law.)
3. Distorted plagioclase crystal in which the relationship of the twinning to the distortion indicates a late age for the formation of the twin lamellae. (Duluth gabbro. ×32. An_{60}. Albite twin law.)
4. Fractured plagioclase which, as in Figure 1, indicates the late age of twin development. (Gabbro, Hartz, Germany. ×400. An_{80}. Albite twin law.)
5, 6. Plagioclase crystals, shattered and recemented with feldspathic material. Cementing material is untwinned. Deuteric minerals are associated with this untwinned feldspathic cement.
   (5—Duluth gabbro. ×48. An_{60}. Albite-Ala twin law.)
   (6—Isle of Mull gabbro. ×90. An_{42}. Albite twin law.)
7. Distorted plagioclase similar to those of Figures 2 and 3. Such crystals are so common and have twinning so similar to undistorted adjacent crystals that similar late ages for twin development are entirely reasonable for other crystals in the rock. (Duluth gabbro. ×32. An_{60}. Albite twin law.)
8. Untwinned plagioclase phenocryst in fine-grained matrix. Lack of twinning in this crystal we attribute to the inability of *small* growing crystals to create suitable stress on the large crystal as crystals of equivalent size could do. (Yellowstone Park basalt. ×90. An_{70}.)

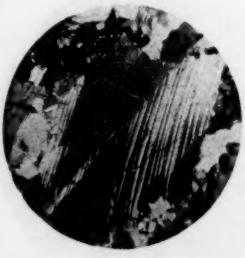
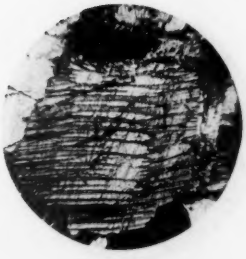


FIGURE 1



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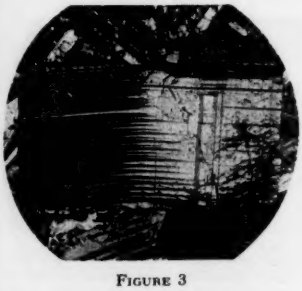

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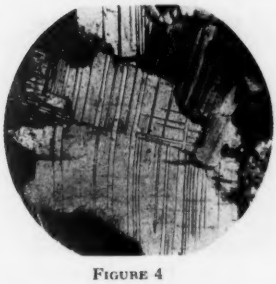

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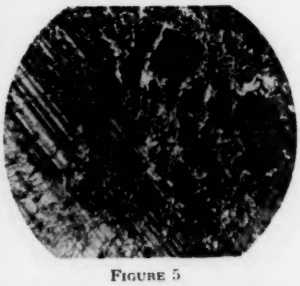

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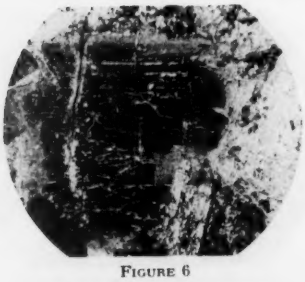

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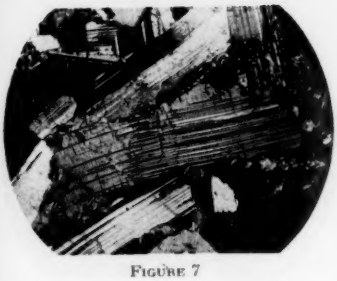

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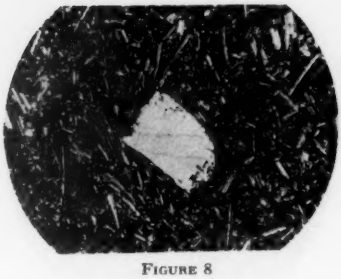
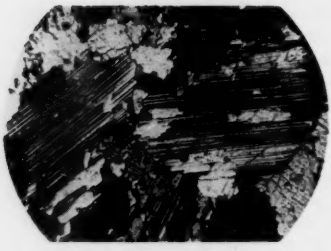


FIGURE 8

SELECTED PLAGIOCLASE TWINS

FIGURE 1

FIGURE 2



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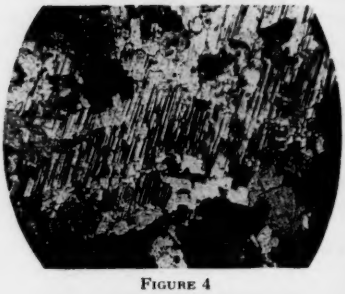

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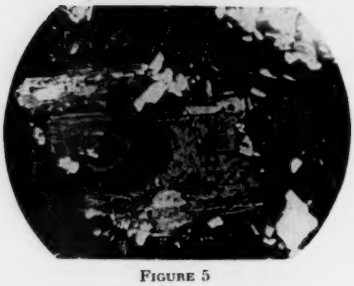
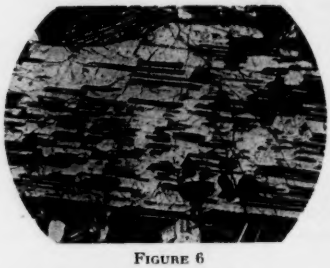


FIGURE 5



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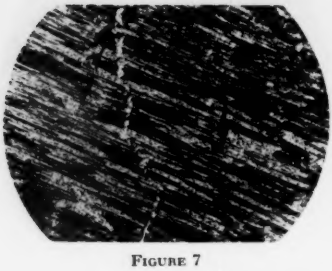

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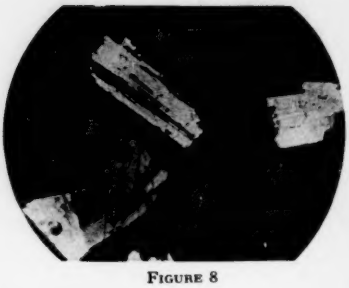


FIGURE 8

SELECTED PLAGIOCLASE TWINS

Plate 2.—Selected Plagioclase Twins

Figure

1. As in Figure 8 of Plate 1, plagioclase phenocryst in fine-grained matrix, almost free of twinning. Such crystals strongly refute the theory of coincidental nature of twinning. (Basalt porphyry, Erzgebirge, Germany. ×16. An_{89}. Albite twin law.)
2. Shattered and twinned phenocryst in basalt which shows flowage structure. (*See* Pl. 3, fig. 4 for lower magnification.) We attribute this twinning to stress of flow, in contrast to Figure 1. (Basalt, Mt. Etna. ×60. An_{68}. Albite twin law.)
3, 4. Fine discontinuous twin lamellae in a metagabbro. Such twinning is found in rocks which show more marked disturbance. (Gabbro, Highland, Mont. 3-×90. 4-×150. An_{70}. Albite twin law.)
5. Virtually untwinned phenocryst in glass. *Compare* with Figure 1. Glass, like a felsitic groundmass, does not induce twin development by mutual crystal interference. (Dacite, Lassen Peak. ×48. An_{31}. Albite twin law.)
6. Extremely discontinuous twinning, which may be regarded as extreme development of twinning of Figure 4. (Gabbro, Thunder Bay, Ont. ×90. An_{55}.)
7. Very fine twin lamellae in highly sodic albite. This is probably albitised feldspar of more calcic original composition. (Pegmatite, Duluth gabbro, near the tunnel west of Duluth. ×200. An_{2-3}. (by Fedorow stereogram). Albite twin law.)
8. Twinning of crystals suspended in glass, we believe to be caused by vesiculation of glass. There is some vesiculation in Figures 5 and 7. We should not expect this result from vesiculation in a glass of low viscosity or from vesiculation sufficiently violent to shatter the phenocrysts. (Basaltic scoria, Hawaii. ×90. An_{80}. Albite twin law.)

Plate 3.—Selected Plagioclase Twins

Figure

1. Shattered zone showing associated fine twinning. (Anorthosite, Beaver Bay, Minn. ×20. An_{50}.)
2. Twinning in plagioclase crystals of equal size. We attribute such twinning mainly to mutual interference of growing crystals. (Anorthosite, Beaver Bay, Minn. ×90. An_{70}.)
3. Gabbro crystal (right An_{55}.) growing into a granite dike (left portion An_{18}.). Twinning of the calcic crystal is continued into the sodic part without apparent modification. (Gabbro-granite, Mellen, Wis. ×48. Albite twin law.)
4. Crystals in basalt shattered by violent flow. Such violence surpasses in part the ability of the crystal to accommodate the stresses by twinning. One of the central crystals here is enlarged in Figure 2 of Plate 2. (Basalt, Mt. Etna. ×16. An_{65}.)
5. Irregular twinning, possibly resulting from stress under conditions unfavorable to and later than the "normal" twinning which the crystal also shows. This irregular twinning is commonly described as strain shadows. (Gabbro, East of Duluth, Minn. ×48. An_{60}.)
6. Plagioclase crystal from a granite hybrid of northern Wisconsin (Marinette Co.). Alternate twin lamellae are selectively sericitzed, conclusive proof that adjacent twin lamellae have different compositions, which difference may be extended easily to include the anorthite content as is indicated by optical data. × 150. An_{20}. Albite twin law.
7. Somewhat discontinuous twinning in rock of mixed composition (mafic gabbro) as compared to the monomineralic rock shown in Figure 2. (×90. An_{70}.)
8. Shattered and twinned crystal showing the effect of intense deformation. (Augen granite gneiss, Horse Race Rapids, Mich. ×32. $An_{.31}$. Albite twin law.)

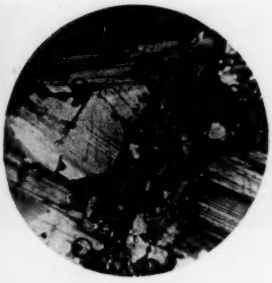


FIGURE 1

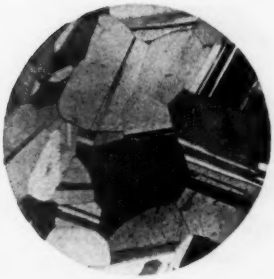


FIGURE 2

FIGURE 3

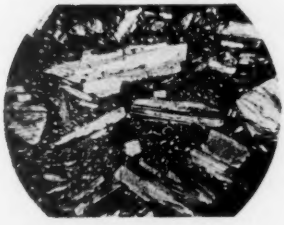
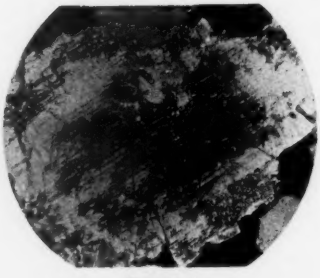


FIGURE 4

FIGURE 5



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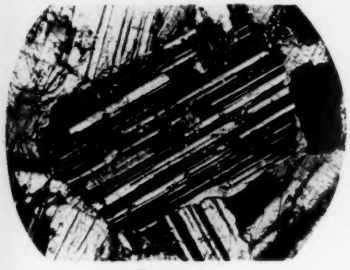

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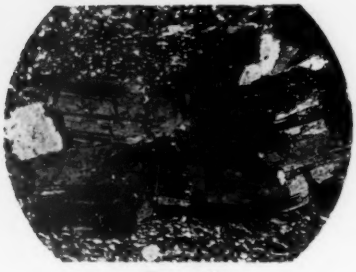

FIGURE 8

SELECTED PLAGIOCLASE TWINS

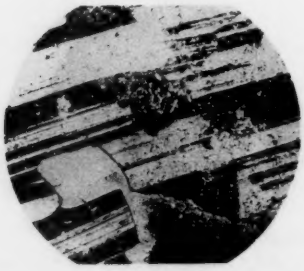

FIGURE 1

FIGURE 2

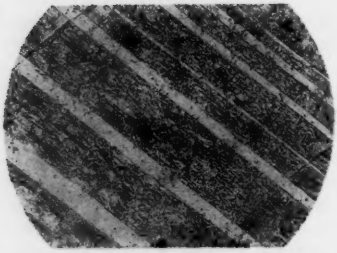

FIGURE 3

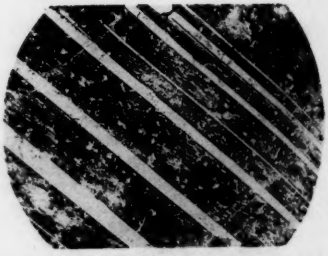

FIGURE 4

FIGURE 6

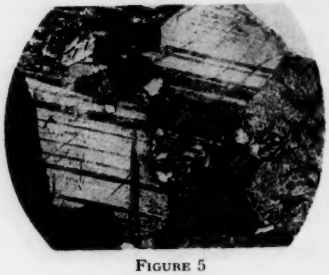

FIGURE 5

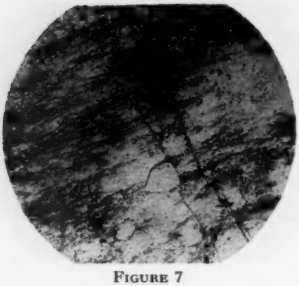

FIGURE 7

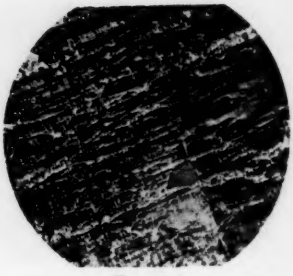

FIGURE 8

SELECTED FELDSPAR TWINS

Plate 4.—Selected Feldspar Twins

Figure

1. Coarse twinning in oligoclase. (Biotite granite. St. George, N. B. ×90. An_{30}. Albite twin law.)
2. Selective replacement of alternate twin lamellae by mica, showing that adjacent twin lamellae may differ in composition. (*See* Pl. 3, fig. 6.) (Oligoclase granite, Apennines. ×90. An_{21}. Ala-A twin law.)
3, 4. Selective replacement by mica of alternate twin lamellae as in Figure 2. (3) ordinary light. (4) crossed nicols. (Granite hybrid, Jayville, N. Y. (×300. An_{28}. Courtesy Dr. S. A. Tyler.)
5, 6. Similar to Figures 3 and 4 but showing the selective replacement in the complex twin relationship parallel to both 010 and 001. (Granite hybrid, Jayville, N. Y. ×90. An_{28}. Albite and pericline twin laws. Courtesy Dr. S. A. Tyler.) Of the many illustrations of this phenomenon seen, those of Figures 3-6 are most striking.
7, 8. Selectively sericitized perthite crystal offered in support of the explanation presented in the text. (Northern Wisconsin. ×64.)

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HELIUM AGE MEASUREMENT. I. PRELIMINARY MAGNETITE INDEX

BY PATRICK M. HURLEY AND CLARK GOODMAN

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ABSTRACT

Helium age measurements on magnetite specimens of various geological ages show a grouping and sequence in accordance with geological knowledge and a spacing compatible with ideas derived from sedimentational history. Ages in millions of years are given as follows: mean Miocene, 19; mean Laramide, 57; mean Nevadan, 118; mean late Triassic, 155; mean late Appalachian, 215; mean Devonian, 348; pre-Cambrian ranging from 500 to 1650. As a result the scale is considered suitable as an index for preliminary investigation of the helium retentivity of minerals and application of the method to igneous rocks.

INTRODUCTION AND ACKNOWLEDGMENTS

Work by the authors (Hurley and Goodman, 1941, p. 545-560) on minerals separated from rocks of late Triassic age gave the following helium age averages:

	Million years
Magnetite	134
Pyroxene	103
Feldspar	36

These discrepancies were assumed to be the result of differential leakage of radiogenic helium from the minerals. Accordingly, it appeared

necessary to discard essentially all the earlier helium age determinations.

Attention was then turned to the question of what minerals, if any, reliably retained all their rediogenic helium. If one mineral could be found that yielded true ages, it would provide an index scale that would make possible a study of the comparative retentivities of other minerals and, eventually, the possibility of applying the method to common igneous rocks. It was decided that the investigation should be as free as possible from any pre-existing ideas of geologic age. With no standard for comparison the only way a mineral might first be shown to yield helium ages approximating the truth was by investigation of the following:

(1.) Agreement in helium ratio [1] between specimens of the same geologic age despite varying, absolute uranium, thorium, and helium contents.

(2.) Agreement in helium ratio between specimens of the same geologic age despite varying ratios of uranium to thorium.

(3.) In a number of specimens of different age a sequence in the helium ratios compatible with the geological knowledge of the actual age sequence.

(4.) A time spacing between specimens of different age reasonably compatible with a knowledge of the historical events within the spaces.

(5.) The assumption that failure of a mineral to be suitable for helium age measurement is due to nonretention of helium by the crystal structure, so that in general minerals showing consistent high helium ratios for their age are more likely to be correct than minerals with lower ratios.

Since magnetite appeared to have the highest helium retention in the Triassic minerals tested, it was chosen for the first work. Specimens of magnetite were collected from deposits or magnetite-rich rocks of various ages. No attempt was made to achieve accuracy in the dating beyond knowledge of the orogeny or period of activity with which each deposit was associated, since the question involved differences in retention up to several fold rather than a few per cent.

After a number of determinations magnetite appeared to satisfy the first four of the above points. In regard to the fifth point a small amount of work on other minerals showed none to yield consistent age ratios higher than magnetite, and measurements on a few sulphides yielded age ratios apparently equal to magnetite. Thus it appears that magnetite may yield ages approximating the truth and that a scale of magnetite helium ages may serve as a temporary standard or index

[1] Helium ratio is equivalent to the "measured helium age." It may or may not be the true age.

permitting future work on the ultimate problem, a method for the age determination of common rocks.

The list of age measurements in Table 1 should not be considered as an attempt at a time scale. The geologic ages of the specimens used are not accurately enough known, the measurements were not accurately enough made, and, although it appears fairly certain that magnetite retains most of its helium, there is still the possibility that it does not retain entirely all of it.

The authors wish to thank Professor W. J. Mead for his sustained interest in this work and Mr. Richard E. Honig for his technical assistance. For most of the specimens, they are indebted to Professor W. H. Newhouse and the Smithsonian Institution. This research has been supported by The Geological Society of America through a grant-in-aid.

AGE MEASUREMENTS

All specimens measured are listed in Tables 1 to 3. After most of them had been measured it was discovered that minute quantities of certain silicate minerals significantly affected the measured age. If the silicate is highly radioactive and has a low helium retentivity, the presence of 1 or 2 per cent in the sample, determined microscopically, may cause an error of several per cent in the age. Many magnetites, originally separated magnetically from gangue minerals, required more careful cleaning. The helium, radium, and thorium measurements made before this final purification have been included in Table 1 to indicate the effect of small quantities of gangue as well

TABLE 1.—*Preliminary magnetite index*

Specimen number	Locality	Helium in 10^{-5} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-6} gm/gm	Geologic age	Helium age (millions of years)
1.	Desert Mound, Utah.....	0.46 0.67 0.67 **0.60** Av.	**0.67**	4.2 4.5 **4.4** Av.	Miocene (?).....	17
2.	Chesapeake, Utah.....	**1.00**	1.02 1.12 1.10 1.14 **1.09** Av.	9.2 11.7 **10.5** Av.	Miocene (?).....	15
3.	Great Western, Utah.....	**0.80**	0.82 0.99 1.09 1.08 **1.00** Av.	6.5 6.7 7.0 **6.7** Av.	Miocene (?).....	15
4.	Black Magnetic, Utah....	**0.82**	0.88 0.98 0.87 **0.91** Av.	2.6	Miocene (?).....	21
5.	Snoqualmie Pass, Wash...	†**0.21**	0.21 0.23 **0.22** Av.	**0.36**	Miocene.....	25

TABLE 1.—*Preliminary magnetite index*—Continued

Specimen number	Locality	Helium in 10^{-6} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-6} gm/gm	Geologic age	Helium age (millions of years)
6.	Sierra de Carrizal, Mex...	**0.43**	0.26 0.25 **0.26** Av.	**0.30**	Mid-Tertiary.....	45
7	Haiti (magne te).....	**0.51**	0.33 0.33 **0.33** Av.	**<0.1**	Late Eocene or Oligocene..	46
8.	Haiti (sulphide).....	0.21 0.16 †**0.19** Av.	**0.13**	**<0.1**	Late Eocene or Oligocene..	43
9.	Fierro, N. M.....	*0.30 *0.30 †**0.16**	*0.13 **0.072**	*0.75 **0.20**	Laramide.....	53
10.	Bingham, Utah (galena)..	**0.69**	0.42 0.38 **0.40** Av.	**<0.1**	Laramide.....	51
11.	Calumet, Colo.....	†**0.14**	0.065 0.045 **0.055** Av.	**<0.1**	Laramide.....	75
12.	Stoddard, Colo..... (impure)	0.79 0.61 **0.70** Av.	**0.27**	1.7 1.5 **1.6** Av.	Laramide.....	>51
13.	Whitehall, Mont..... (chalcopyrite)	**3.31**	**2.0**	**0.1**	Laramide.....	50
14.	Magnet Cove, Ark.....	†**0.17**	**0.08**	**<0.1** **<0.1** **<0.1** Av.	Mid-Cretaceous (?).....	63
15.	Beshi, Japan.....	**0.78**	0.22 0.29 **0.26** Av.	**<0.1**	Mesozoic (?).....	90
16.	Texada Island, B. C..... (MIT Spec.)	1.66 1.97 1.68 **1.77** Av.	0.35 0.48 **0.42** Av.	**0.35**	Nevadan.....	117
17.	Texada Island, B. C..... (Sm. Spec.)	**2.35**	0.58 0.58 **0.58** Av.	**<0.1**	Nevadan.....	120
18.	Lynn Valley, B. C.....	*1.26 *1.51 *1.76 **1.70**	*0.43 **0.49**	**0.28**	Nevadan.....	100
19.	Butte County, Calif.....	†**0.22**	0.035 0.02 0.02 **0.024** Av.	†**0.36**	Nevadan (?).....	120
20.	Juragua, Cuba.....	0.22 0.23 †**0.22** Av.	**0.066**	**<0.1**	Nevadan (?).....	102
21.	Clipper Gap, Calif.....	†**0.34**	0.083 0.070 **0.076** Av.	**<0.1**	Nevadan.....	132
22.	Prince of Wales Island, Alaska.	*3.53 **5.00**	*1.43 *1.71 *1.50 **1.08**	**0.52**	Nevadan.....	132

TABLE 1.—*Preliminary magnetite index*—Continued

Specimen number	Locality	Helium in 10^{-6} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-6} gm/gm	Geologic age	Helium age (millions of years)
23.	Goose Creek, Va. (impure)	*1.57	*0.15	*2.3	Late Triassic	>140
			*0.16	*2.2		
		1.99				
			0.24	2.2		
24.	French Creek, Pa. (impure)	0.86	0.09	1.19	Late Triassic	>140
			0.09	1.17		
				0.91		
			0.09 Av.			
				1.10 Av.		
25.	Boyertown, Pa. (impure)	1.76	0.29	1.30	Late Triassic	>152
		2.01	0.23	0.77		
			0.29			
		1.88 Av.	0.33	1.03 Av.		
			0.28 Av.			
26.	Dillsburg, Pa. (impure)	0.74	0.14	0.38	Late Triassic	>143
			0.10			
			0.12 Av.			
27.	Preakness Mt., N. J. (impure)	1.69	0.28	0.58	Late Triassic	>153
28.	Lakeville, N. S.	*1.14	*0.16	*0.60	Late Triassic	169
				*0.77		
		1.37	0.22			
				0.30		
29.	Gerrish Mt., N. S.	*0.78	*0.05	*1.6	Late Triassic	158
				*1.4		
		0.74	0.045			
				1.1		
30.	Cashtown, Pa.	*5.27	*0.82	*1.1	Late Triassic	174
		*4.82		*0.8		
			0.90			
		5.57		0.53		
		5.61		0.56		
		5.59 Av.		0.55 Av.		
31.	Virginia diabase	2.42	0.17	3.10	Late Triassic	165
32.	Rammelsberg, Germ. (pyrite)	4.72	0.61	0.41	Late Upper Carboniferous	200
			0.74			
			0.68 Av.			
33.	Trun, Spain	1.42	0.16	0.62	Late Upper Carboniferous (?)	205
			0.15			
			0.15			
			0.15 Av.			
34.	Oviedo, Spain	10.5	0.67	10.6	Late Upper Carboniferous (?)	200
35.	Chester, Mass.	†0.20	0.021	0.05	Late Upper Carboniferous (?)	225
36.	Ducktown, Tenn. (pyrrhotite)	*4.38	*0.44	1.07	Late Upper Carboniferous (?)	215
	(Burra Burra)	3.59	*0.42			
			0.40			
37.	Ducktown, Tenn. (pyrrhotite)	*2.07	*0.18	0.43	Late Upper Carboniferous (?)	245
			*0.26			
		2.20				
			0.22			
38.	Magnitnaya, Urals (MIT Spec.)	4.63	0.49	0.20	Late Lower, or Upper Carboniferous	240
		3.11	0.41			
		4.14				
		3.62	0.45 Av.			
		3.42				
		3.78 Av.				
39.	Magnitnaya, Urals (Sm. Spec.)	6.1	0.71	0.29	Late Lower, or Upper Carboniferous	245
		6.0				
		6.0 Av.				

TABLE 1.—*Preliminary magnetite index*—Continued

Specimen number	Locality	Helium in 10^{-5} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-4} gm/gm	Geologic age	Helium age (millions of years)
40.	Vysokaya, Urals	0.81 0.80 **0.80** Av.	**0.05**	0.33 0.13 **0.23** Av.	Mid-Devonian (?)	340
41.	Blagodat, Urals (MIT Spec.)	*1.38 *1.68 *1.61 **1.24**	*0.10 **0.081**	0.21 0.25 **0.23**	Devonian (?)	365
42.	Blagodat, Urals (Sm. Spec.)	*2.50 *2.63 **1.42**	*0.16 *0.17 **0.11**	**0.26**	Devonian (?)	340
43.	Salem, Mass (impure)	**0.86**	**0.047**	**0.35**	Post-Cambrian, pre-Devonian	>330
44.	Lökken, Norway (pyrite, pyrrhotite)	3.17 3.20 **3.18** Av.	0.24 0.22 0.21 **0.22** Av.	**0.22**	Late Silurian	390
45.	Mellen, Wis	**3.28**	**0.13**	†**0.72**	Upper Keweenawan	500
46.	Eagle River, Mich	**3.04**	**0.10**	†**0.67**	Middle Keweenawan	550
47.	Murray Mine, Sudbury (pyrrhotite)	**3.59**	0.09 0.10 **0.096** Av.	**1.06**	Middle Keweenawan	550
48.	Copper Cliff, Sudbury (pyrrhotite)	**0.76**	0.034 0.023 0.022 **0.026** Av.	†**0.13**	Middle Keweenawan	620
49.	Salisbury, N. Y.	**7.25**	**0.23**	**1.94**	Late pre-Cambrian	540
50.	Ford Mine, N. J.	**3.95**	**0.076**	**1.62**	Late pre-Cambrian	550
51.	Ringwood, N. J.	*25.5 **10.6**	*1.65 **0.27**	*15.7 **5.9**	Late pre-Cambrian	410
52.	Cheever, Mineville, N. Y.	*19.3 **5.33**	*2.30 †**0.016**	*25.0 **3.1**	Late pre-Cambrian	560
53.	Salisbury, N. Y.	**4.36**	**0.16**	**1.15**	Late pre-Cambrian	500
54.	Franklin Furnace	**0.66**	0.019 0.046 0.024 **0.029** Av.	<**0.02**	Late pre-Cambrian	640
55.	Edwards, Mich	34.2 42.1 35.5 **37.3** Av.	0.56 0.64 **0.60** Av.	2.15 1.76 **1.95** Av.	Late Middle Huronian (?)	1300
56.	Republic, Mich	*27.6 **17.4**	*0.30 *0.31 **0.30** Av.	*0.90 **1.13**	Late Middle Huronian (?)	1200
57.	Spur Mine, Mich	*2.54 **2.39**	*0.077 *0.079 **0.074**	*0.25 **0.15**	Late Middle Huronian (?)	800
58.	Michigamme, Mich	*14.3 **8.65**	*0.14 *0.14 **0.13**	*0.87 **0.40**	Late Middle Huronian (?)	1400
59.	Champion, Mich	*8.76 *9.56 **8.52**	*0.135 *0.137 **0.094**	*0.52 **0.40**	Late Middle Huronian (?)	1650
60.	Humboldt, Mich	65.0 68.0 **66.6** Av.	1.06 0.98 1.14 1.08 0.76 **0.98** Av.	**0.24**	Late Middle Huronian (?)	1650

TABLE 1.—*Preliminary magnetite index*—Continued

Specimen number	Locality	Helium in 10^{-4} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-4} gm/gm	Geologic age	Helium age (millions of years)
61.	Giant's Range, Minn. (impure)	**73.3**	**1.00**	**18.7**	Algoman (?)	>800
62.	Cranberry, N. C.	**1.50**	**0.038**	**0.18**	Pre-Cambrian	830
63.	Cranberry, N. C.	**2.76**	0.07	<0.1	Pre-Cambrian	1260
			0.06			
			0.06 Av.			
64.	Llano County, Texas	**4.36**	0.05	0.87	Pre-Cambrian	1050
		5.32	0.06	1.13		
		5.38		0.86		
			0.05 Av.			
		5.09 Av.		**0.95** Av.		
65.	Kiruna, Sweden	65.0	0.30	16.2	Pre-Cambrian	1000
		62.0	0.31	12.0		
		63.0	0.30	20.2		
			0.30 Av.	24.0		
				18.0 Av.		
66.	Kiruna, Sweden	10.46	0.10	1.4	Pre-Cambrian	1325
		8.20	0.10	1.0		
		10.00	0.09	1.2		
		9.6 Av.	**0.097**	1.2 Av.		
67.	Dungannon, Ont.	**2.81**	**0.028**	**0.32**	Pre-Cambrian	1450
68.	Cumberlandite, R. I.	**3.7**	**0.024**	**0.50**	Pre-Cambrian	1500
69.	Grängesberg, Sweden	18.5	0.09	2.4	Pre-Cambrian	1650
		17.4	0.10	2.5		
				2.4		
		18.0 Av.	**0.095** Av.	2.4		
				2.4 Av.		
70.	Falun, Sweden	**37.9**	0.55	<0.1	Pre-Cambrian	1650
			0.58			
			0.56 Av			

Bold face figures were used in computing the age values.

* Analyses made before final separation of gangue. *See* page 313.

< Designates "less than".

> Designates "greater than". Where a sample has been marked "impure" it is assumed that the age measurement will be slightly lower than the value for the pure magnetite.

† Measurements too low for accuracy.

as to provide a knowledge of the reproducibility of analytical results on separate portions of the same sample. It is not known what fraction of the spread of results on repeated measurements of a single sample is due to experimental uncertainty and how much is the result of inhomogeneity of the sample.

Age determinations on 70 samples (Table 1) show a grouping and sequence in accordance with their geological age, and a spacing of the groups compatible with sedimentational history. In most of the groups there is a considerable variation between specimens in their values of helium and radioactivity, while the ratio of helium to radioactivity remains almost constant. Furthermore, in several cases two specimens in the same group will show approximately the same age despite different radium-thorium ratio. A total of 82 acceptable samples were analyzed, of which only 12 did not readily fit the

place indicated by geological knowledge, and the geology of four of these is too uncertain to contradict the helium age. In every case the questionable result may be due to recrystallization of the magnetite or to inclusion of primary helium. The anomalous results are listed in Tables 2 and 3. With little geological knowledge to indicate the age of the pre-Cambrian specimens it is not known whether the age measurements are anomalous. Unless they have shown ages fitting into the post-Proterozoic section of the scale, or else are extraordinarily high, they have been included in Table 1. Little weight should be given to individual measurements in the pre-Cambrian. Eventual pre-Cambrian correlation work should be based on extensive study of each province. These preliminary measurements on pre-Cambrian specimens were made to observe if a reasonable span of time would be disclosed.

DISCUSSION OF RESULTS

GENERAL STATEMENT

Two important requirements must be met by any magnetite sample to be tested by the helium method: (1) It must have sufficient radioactivity and helium to be within the range of accurate measurement; and (2) It must be easily separable from associated minerals.

RANGE OF MEASUREMENTS AND ACTIVITIES

A forthcoming paper in this series by the authors will describe the techniques and precautions involved in the measurement of quantities of helium, radium, and thorium well below the average values for terrestrial materials. Since the quantity of helium is linked to the radium and thorium content and increases with the age of the material, the measurement of the low helium content is the important factor in young ages. This position is reversed for older ages, and the quantity of radium and thorium dictates the lower limit of measurement. Under the present limits of accuracy, the emphasis swings from helium to radioactivity near the Jurassic. As far as significance in helium production is concerned radium and thorium can be measured with approximately the same experimental uncertainty. For the results herein reported the lower limit of thorium measurement is 0.1×10^{-6} gm Th/gm, which is equivalent in helium production to 0.012×10^{-12} gm Ra/gm, the approximate lower limit for radium. Increased accuracy has been achieved recently.

Whether the ultimate accuracy of the measurement of magnetite age is due to helium loss or analytical and sampling uncertainty is an important question. The authors feel that there is no reason to think that magnetite does not retain exactly 100 per cent of its helium. The sampling error, due to inhomogeneity of the material, and analytical uncer-

tainty are both reduced as the measurements are repeated. Future improvement in the technique of low helium measurement may modify the index ages in the Tertiary group.

EFFECT OF IMPURITIES

In the initial measurement of magnetite ages, a rapid survey was made without too much attention to detail. Specimens that appeared macroscopically as massive magnetite were ground and used directly as samples. Impure specimens were passed below a magnet, and the crude magnetic concentrates were used as the samples. When it became apparent that a scale based on magnetite could probably be used as an index, the purity of the earlier samples was examined microscopically. Many had to be ground further to unlock grains of gangue and then reseparated. Some were so intimately associated with gangue that the samples were discarded. All measurements have been included in Table 1, both before and after this final separation. In most cases the early samples were more than 90 per cent magnetite. The final samples are generally better than 99 per cent magnetite. The effect of this small percentage of gangue can thus be observed.

In the magnetite from New Jersey and New York, the Ringwood, Ford, Cheever, and Salisbury specimens, the small quantity of gangue made a difference in the age results of more than a factor of 2. For example, in the Cheever Mine sample, the gangue contained 79×10^{-5} cc He/gm, 12×10^{-12} gm Ra/gm, and 120×10^{-6} gm Th/gm, that is, more than 75 times the activity of the magnetite. The final sample was 99.7 per cent pure by count under the microscope, and the age was adjusted for the 0.3 per cent gangue.

FUTURE APPLICATIONS

GENERAL STATEMENT

The accurate measurement of low quantities of the radioactive elements much beyond the limits of the spectroscopic measurement of other minor impurities opens up many fields of investigation in which these elements may be used as tracers in the study of geologic processes. Although to a large extent the result of measuring techniques developed primarily for study of age determination, these fields may become more important than the parent research.

Considering here only the possible future of helium age determination, and particularly its limitations, it appears at present as if useful application may develop under these general heads: (1) the approximate determination of the geologic age of unknown unmetamorphosed igneous rocks or minerals; (2) the establishment of a fairly accurate geologic

time scale; (3) large-scale correlation in the history of the pre-Cambrian. These are considered separately below, in the light of present knowledge.

APPLICATION TO IGNEOUS ROCKS

It is questionable whether accurate determinations may ever be made of the age of igneous rocks in general. However, there is some promise that for unmetamorphosed igneous rocks in many cases close approximations may be obtained, and in most cases at least an indication within one or two geologic periods. Two methods of analysis require investigation.

First, if a clean separation of accessory magnetite can be obtained from the rock the age may be measured on this separately. Preliminary attempts at this are shown in the case of specimens 23, 27, 31, 43, 45, 46, and 61 in Table 1. This preliminary work did not include a rigorous attempt at obtaining a clean separation. The rock was ground to pass 200 mesh and the magnetite separated by magnet in air or water. Microscopic examination of the concentrate showed that a clean separation would be difficult if not impossible in most cases, unless new techniques are developed. However, the gangue content may be measured, and the effect of the impurity may be determined by separate analysis of the impurity.

This method may yield age determinations easily and fairly accurately on certain rock types, but the study is beset with difficulties and should be made only by a careful petrographer. There is a lower limit to the size of magnetite grain permissible, about 30 microns, set by the range of the alpha particles in the mineral. There is a possibility that the magnetite, if late in the crystallization of the rock, may be intimately associated with other late material enriched in uranium and thorium. Difference in radioactivity between the magnetite and neighboring minerals increases the difficulties in correcting for imperfect separation of the magnetite. Furthermore, if crystallization takes place under high pressure and in the presence of gases containing helium from pre-magmatic radioactive disintegration, the magnetite, or accompanying unseparated material, may contain "primary helium". This is believed to be the cause of the anomalous helium ratios in Table 2. Another possible explanation of these high helium contents is the coprecipitation with the magnetite of short-lived radioactive elements that have disintegrated to lead but whose helium has been added. Radium is the only short-lived element that appears at all possible for this since elements preceding radium and thorium X in the radioactive series would mean precipitation of the isotopic parents, U and Th, and an improbable quantity of the remaining transitory elements would be required to supply the helium

excesses found. Extrusive rocks crystallizing under low pressure have not contained "primary helium". With these, however, the magnetic concentrate may contain mixtures from fairly pure magnetite to iron-rich base material in which the magnetite may occur in dustlike particles or dendritic growths impossible to separate.

TABLE 2.—*Measurements showing possible primary helium*

Sample Number	Locality	Helium in 10^{-5} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-6} gm/gm	Helium age (millions of years)
71	Accessory magnetite from Sudbury olivine diabase.	34.8	0.08	0.24	10,000
72	Accessory magnetite from Sudbury norite.	120.0	0.10	.....	<36,000
73	Worthington mine, Sudbury (pyrrhotite).	3.80	<0.01	<0.1	>12,000
74	Frood mine, Sudbury (pyrrhotite).	2.84	0.018	<0.1	4,700
75	Caribou, Colo.....	4.10	0.02	<0.1	5,700

Despite these difficulties age measurements on separated accessory magnetite have shown encouraging agreement with measurements on massive magnetite of the same age.

The second possibility of measuring the approximate age of igneous rocks lies in the question of whether nonretentive minerals still retain a fairly constant proportion of their helium. There is some indication that this is true. If so, minerals separated from the unknown rock may be measured, and their helium loss compensated. In the case of extrusive rocks where groups have similar mineral composition and conditions of crystallization it may be possible to analyze the rock as a whole and multiply the result by some factor predetermined for each group to arrive at an approximate age. This was suggested by the measurement of several different basalts. In each case, if the measured age were multiplied by the factor 2.2, the resultant age was equal to the age as given by the magnetite index scale. This factor 2.2 agrees approximately with a factor to be expected from a feldspar retentivity of 0.24 and a pyroxene retentivity of 0.70 as found by the measurement of minerals separated from the Palisade diabase. The distribution of radioactive elements between the minerals in rocks of a single group will probably be fairly constant. Such a method would be used only where rough determinations would be of value.

GEOLOGIC TIME SCALE

The helium method shows more promise in the establishment of a fairly accurate geologic time scale. In this case material most suitable for measurement may be sought. It is to be expected that this work will not approach completion for a long time. The collection of material both accurately dated geologically and suitable for measurement will be slow. Work should not be started on a time scale until more is known of the retentivities of minerals, the cause and frequency of "primary helium", and the reasons behind analytical fluctuations.

PRE-CAMBRIAN CORRELATIONS

Perhaps the most useful future application of the helium method may lie in additions to the history of the pre-Cambrian and in the correlation of disconnected pre-Cambrian sections. Since pre-Cambrian history has been developed largely by the relationships between igneous rocks the dating of igneous groups and associated deposits would be of particular value. Material suitable for the helium method seems to be abundant, although not enough work has been done to show how much difficulty

TABLE 3.—*Discordant results*
In most cases probably due to recrystallization

Specimen Number	Locality	Helium in 10^{-6} cc/gm	Radium in 10^{-12} gm/gm	Thorium in 10^{-6} gm/gm	Probable geologic age	Helium age (millions of years)
76	Bancroft, Ont.....	2.49	0.137	1.37	Pre-Cambrian	300
77	Craigmont, Ont....	1.03	0.051	<0.1	Pre-Cambrian	600
78	Monmouth, Ont....	0.85	0.05	0.25	Pre-Cambrian	370
79	Ketchikan, Alaska..	1.39	0.13	0.34	Nevadan	270
80	St. Charles, Que.... Titaniferous magnetite	0.16	0.02	<0.7	Pre-Cambrian	<200
81	Broken Hill, N.S.W. Galena	<1.2	1.0	.....	Pre-Cambrian	<30

will arise from recrystallization in these older rocks and minerals (Table 3). It may be necessary to analyze a number of specimens from each group to gain information on the acceptability of the results. The measurements on pre-Cambrian material in Table 1 show considerable variation between specimens of the same age, but the general span of ages obtained is encouraging. If enough work is done on any single pre-Cambrian epoch without placing much weight on individual measurements and by working on more than one mineral type at once, an acceptable age result will appear.

CONCLUSIONS

Results of the investigation are:

(1) Accurate analysis of the helium, radium, and thorium content of cleanly separated magnetite will give the length of time since the formation of the present crystal structure of the magnetite. A fairly rare exception is the case in which apparently helium was trapped in the structure of the magnetite at the time of formation.

(2) Minerals other than magnetite may be found suitable. Work has been insufficient to draw definite conclusions, but a few measurements suggest that the sulphides, pyrite, pyrrhotite, and galena are reliable. However, they are much more likely to have recrystallized since they were originally formed than magnetite.

(3) Age measurements made directly on samples of igneous rocks are unsuccessful owing to leakage of helium from most of the common rock minerals. Successful age measurement of igneous rocks may be accomplished eventually by separation of minerals that are known to retain all, or a constant definite fraction, of their radiogenic helium.

GEOLOGIC NOTES ON ROCK SPECIMENS REFERRED TO IN TABLES

1. Desert Mound, Iron Springs, Utah. Magnetite ore. Newhouse Collection No. 7888. Partially oxidized siderite gangue. Magnetic concentration.
   Age: Probably Miocene. Deposits associated with post-Eocene andesite which is similar to Miocene lavas of Wasatch Mts. (Leith and Harder, 1908, p. 46.)
2. Chesapeake Mine, Irontown, Iron Co., Utah. Magnetite ore. Smithson. Inst. Coll. No. 42182. Dense, fine-grained magnetite.
   Age: Probably Miocene. *See* Specimen No. 1, Desert Mound.
3. Great Western Mine, Iron Springs, Iron Co., Utah. Magnetite ore. Smithson. Inst. Coll. No. 42174. Dense, fine-grained magnetite.
   Age: Probably Miocene. *See* Specimen No. 1, Desert Mound.
4. Black Magnetic Mine, Irontown, Iron Co., Utah. Massive magnetite ore. Smithson. Inst. Coll. No. 42177.
   Age: Probably Miocene. *See* Specimen No. 1, Desert Mound.
5. Near Snoqualmie Pass, King Co., Wash. Magnetite coarsely associated with garnet. Smithson. Inst. Coll. No. 62393. Magnetic concentration.
   Age: Probably Miocene. Probably associated with Snoqualmie granodiorite batholith (Smith and Mendenhall, 1900, p. 224) which cuts Miocene lake beds but is older than Quaternary andesites.
6. Sierra de Carrizal, Estado de Nuevo Siam, Mexico. Smithson. Inst. Coll. No. 91529. Magnetite coarsely associated with calcite. Magnetic concentration.
   Age: Probably about middle Tertiary. W. Foshag (personal communication): associated with granitic instrusions cutting Cretaceous sediments; same group of intrusives associated with uraninite of Virgin Mine, Placer de Guadalupe, which was analyzed by Wells (1930, p. 470) and gave a Pb/U+0.36Th ratio of 0.0046, equivalent to an age of about 35 million years.
7. Germain Tunnel, Mémé T. N., Haiti. Specimen consisted of large masses of magnetite in a gangue of coarsely crystalline calcite. Magnetite is apparently pseudomorphic after hematite. Newhouse Collection No. 6770.
   Age: Tertiary, probably Ecocene or Oligocene. Deposits associated with intrusions cutting early Tertiary beds. (Geol. Survey, Republic of Haiti, p. 443.)

8. La Cascade, Mémé T.N., Haiti. Coarse aggregate of pyrite, garnet, calcite, and quartz. Picked concentrate of sulphide so that sample consisted of more than 95 per cent sulphide under the microscope.
Age: Tertiary, probably Eocene or Oligocene. *See* Specimen No. 7, Germain Tunnel.

9. Fierro, N. Mex. Magnetite ore. Newhouse Coll. No. 7405. Specimen appears to have suffered from shear stress.
Age: Probably early Tertiary. Deposit associated with porphyry belonging to a group of intrusive rocks placed as latest Cretaceous or, more probably, early Tertiary by Lindgren, Graton, and Gordon (1910, p. 35.).

10. Utah Apex Mine, Bingham Canyon, Utah. Newhouse Coll. No. 3205. Specimen of galena.
Age: Early Tertiary, probably Eocene.

11. Calumet Mine, Custer Co., Colo. Smithson. Inst. Coll. No. 5432. Medium-grained magnetite ore, with disseminated, low temperature gangue.
Age: Probably associated with late Cretaceous and early Tertiary activity.

12. Stoddard Mine, Costilla Co., 5 miles from Placer, Colo. Magnetite ore from Upper Tunnel stockpile, Smithson. Inst. Coll. No. 41863. Magnetite ore associated with carbonate gangue. Magnetic concentration.
Age: Probably associated with late Cretaceous and early Tertiary activity.

13. Whitehall, Mont. Last Chance Mine. Massive chalcopyrite. Newhouse Coll. No. 2964.
Age: Laramide, probably early Eocene.

14. Magnet Cove, Arkansas. Massive magnetite.
Age: Probably Mid-Cretaceous (Croneis and Billings, 1929, p. 557).

15. Besshi Mine, Japan. Massive pyrite. Newhouse Coll. No. 6800.
Age: Mesozoic. (Watanabé, M., 1923 p. 174.)

16. Texada Island, B. C. Magnetite ore from the contact of the iron and copper deposits. Newhouse Coll. No. 5943. Magnetic concentration.
Age: Nevadan. Associated with Coast Range batholith.

17. Texada Island, B. C. Magnetite. Smithson. Inst. Coll. No. 42200.
Age: Nevadan. Associated with Coast Range batholith.

18. Lynn Valley, Vancouver, B. C. Magnetite ore. Newhouse Coll. No. 5910.
Age: Late Jurassic or early Cretaceous. Associated with Coast Range granodiorite batholith.

19. Butte Co., Calif. Massive magnetite. Smithson. Inst. Coll. No. 17674.
Age: Late Jurassic or Early Cretaceous. Associated with Nevadan orogeny.

20. Juragua Mines, Cuba. Magnetite ore. Newhouse Coll. No. 8335.
Age: Probably late Jurassic, or else Miocene. (Schuchert, 1935, p. 496.)

21. Clipper Gap Mine, Placer Co., Calif. Coarse-grained magnetite, slightly hematitic. Smithson. Inst. Coll. No. 42279.
Age: Late Jurassic or early Cretaceous. Associated with Nevadan orogeny.

22. Copper Mtn., Prince of Wales Island, Alaska. Magnetite associated with quartz and calcite, and a minor amount of chalcopyrite. Magnetic concentration of magnetite. Smithson. Inst. Coll. No. 91562.
Age: Nevadan. Associated with Coast Range Instrusives. (Buddington, 1927, p. 232.)

23. Goose Creek, Loudoun Co., Va. Specimen of basic pegmatite with bladed augite crystals and basic feldspar. Fairly coarse-grained magnetite as an accessory, was imperfectly separated by magnet.
Age: Late Triassic, associated with typical Triassic diabase intrusive (Shannon, 1925, p. 1.)

24. French Creek, Pa. Magnetite ore. Newhouse Coll. No. 7813. Impure magnetite concentration.
Age: Late Triassic. Magnetite deposit associated with Triassic diabase intrusive.

25. Boyertown, Pa. Magnetite ore. Newhouse Coll. No. 7702. Sample imperfectly separated by magnet from intimately associated gangue.
Age: Late Triassic. Deposit associated with Triassic diabase intrusive.

26. Dillsburg, Pa. Magnetite ore. Newhouse Coll. No. 7793. Sample very impure owing to intimate gangue mixture.
Age: Late Triassic. Associated with Triassic diabase intrusive.

27. Preakness Mt., N. J. Fine-grained diabase of second Watchung group of Triassic extrusives. Rock contained from 1 to 2 per cent magnetite as an accessory. This was concentrated magnetically, yielding an impure sample of magnetite, with probable ilmenite, and a fair amount of iron-rich, cloudy base material.
Age: Late Triassic.

28. Nova Scotia. 2 miles northwest of Lakeville. Magnetite associated with a flow rock. Newhouse Coll. No. 8082.
Age: Late Triassic. Lavas extruded before Palisade disturbance.

29. Gerrish Mtn., N. S. Between Five Islands and Economy, N. S. Magnetite-quartz veinlets in Triassic lava flows. Newhouse Coll. No. 8056. Clean magnetic separation.
Age: Late Triassic. Lavas extruded before Palisade disturbance.

30. Cashtown, Pa. Magnetite ore. Newhouse Coll. No. 7715.
Age: Late Triassic. Deposit associated with Triassic diabase intrusive.

31. Virginia diabase. Triassic diabase chosen by National Bureau of Standards as a rock standard for radioactivity measurement. Accessory magnetite separated magnetically, yielding a fairly pure sample.
Age: Late Triassic.

32. Rammelsberg, Harz Mts., Germany. Fine, dense pyrite, showing an appearance of flowage.
Age: Probably deposited, or recrystallized, in late Hercynian times. Origin disputed: either syngenetic with enclosing Devonian sediments, or of replacement origin associated with later igneous activity. Weight of opinion favors epigenetic origin and associated with near-by granite batholith of late Carboniferous age (Lindgren and Irving, 1911, p. 303).

33. Trun, Guipuzcoa Province, Spain. Magnetite ore. Smithson. Inst. Coll. No. 34228. Dense, fine-grained magnetite with occasional crystal of quartz and minor disseminated pyrite.
Age: Probably late Carboniferous, Hercynian orogeny.

34. Oviedo Province, Spain. Magnetite ore. Smithson. Inst. No. 34217. Fine-grained magnetite with minor chloritic gangue.
Age: Probably late Carboniferous. Associated with Hercynian orogeny (?).

35. Chester, Mass. Magnetite separated from emery.
Age: Late Carboniferous, probably formed by action of late Carboniferous intrusives (Emerson, 1898, p. 17).

36. Burra Burra Mine, Ducktown, Tenn. Newhouse Coll. No. 5642. Specimen consisted mainly of pyrrhotite, with pyrite, quartz, and chalcopyrite. Pyrrhotite was concentrated by use of heavy liquids and Frantz Isodynamic Separator.
Age: Probably late Carboniferous. *See* Spec. No. 37.

37. Ducktown, Tenn. Smithson. Inst. Spec. No. 97516. Specimen consisted of pyrrhotite, pyrite, chalcopyrite, and quartz. A sample of pyrrhotite was obtained by separation with heavy liquids, followed by Frantz Isodynamic Separator.
Age: Probably late Carboniferous (Ross, 1935, p. 24).

38. Magnitnaya, Magnitogorsk, Urals. Magnetite ore. Newhouse Coll. No. 8493. Presence of hematite indicated by reddish color of powder.
Age: Probably Upper Carboniferous. Associated with granitic intrusions cutting Lower Carboniferous strata (Zavaritsky, 1937, p. 92).

39. Mt. Magnitnaya, Magnitogorsk, Urals. Magnetite ore. Smithson. Inst. Coll. No. 103342. Although all magnetic, specimen shows hematite by color of streak.
Age: Upper Carboniferous. *See* Spec. No. 38.

40. Vysokaya, Urals, U.S.S.R. Magnetite ore. Newhouse Coll. No. 8493.
Age: Devonian (?). Deposit associated with syenite intrusion believed by Derwies (1937, p. 39) to be Middle Devonian.

41. Blagodat, Urals, U.S.S.R. Magnetite ore. Newhouse Coll. No. 8493.
Age: Probably Devonian. Same as Spec. No. 40.

42. Blagodat, Urals, U.S.S.R. Magnetite ore. Smithson. Inst. Coll. No. 103318.
Age: Probably Devonian. Same as Spec. No. 40.

43. Salem, Mass. Coarse-grained gabbro-diorite. Rock contained from 1 to 3 per cent accessory magnetite. This was concentrated magnetically, yielding an impure sample of magnetite, and probably associated ilmenite, with a fair amount of cloudy, iron-rich material.
Age: Post-Cambrian, pre-Devonian (?) (La Forge, 1932, p. 22.).

44. Lökken, Norway. Pyrite and pyrrhotite ore. Newhouse Coll. No. 6954.
Age: Late Silurian or early Devonian. (Falkenberg, 1914, p. 107.)

45. Gabbro, 4 miles west of Mellen, Wis. Believed to be contemporaneous with Duluth gabbro. Contained only 2 or 3 per cent accessory magnetite which was concentrated magnetically. Magnetite sample obtained was estimated to be 85 to 90 per cent magnetite (including ilmenite) under microscope.
Age: Upper Keweenawan (Leith, Lund, and Leith, 1935, map). Middle Keweenawan (Tyler, Marsden, Grout, and Thiel, 1940).

46. Lake Shore trap, Eagle River, Mich. Specimen of augite-rich greenstone. Rock was ground to pass 35 mesh and an impure concentration made of accessory magnetite. This concentrate was further ground and separated until under microscope it showed a content of more than 95 per cent magnetite (including ilmenite, if present).
Age: Middle Keweenawan. Overlies great conglomerate of Keweenaw peninsula.

47. Murray Mine, Sudbury district, Ont. Pyrrhotite. Newhouse Coll. No. 8898.
Age: Middle Keweenawan (Cooke, 1941, p. 14).

48. Copper Cliff Mine, No. 4, Sudbury district, Ont. Massive pyrrhotite. Newhouse Coll. No. 8927.
Age: Middle Keweenawan (Cooke, 1941, p. 14).

49. Salisbury Mine, New York. Magnetite ore. Collected by S. A. Tyler, near east end of Main Vein. Massive magnetite with small amount of green silicate gangue and very minor sulphide. Magnetic concentration.
Age: Pre-Cambrian. *See* Spec. No. 52.

50. Ford Mine, north of Stanhope, N. J. Newhouse Coll. No. F-4. Coarse-grained magnetite ore.
Age: Pre-Cambrian. *See* Spec. No. 52.

51. Ringwood, N. J. Coarse-grained, massive magnetite. Newhouse Coll. No. EM-7.
Age: Pre-Cambrian. *See* Spec. No. 52.

52. Cheever Mine, Mineville, N. Y. Newhouse Coll. No. 7510. Coarse-grained magnetite ore.
Age: Pre-Cambrian. Although age of this specimen cannot be given closely by geology, it is of particular interest in that it has a well-established upper limit to its age. Igneous complex in which this deposit occurs is overlain unconformably by Cambrian Potsdam sandstones. This group of New York and New Jersey magnetites therefore indicates a bottom limit to Paleozoic of less than 500 million years.

53. Salisbury Mine, N. Y. Fine-grained magnetite ore, collected by S. A. Tyler from near west end of Main Vein.
Age: Pre-Cambrian. *See* Spec. No. 52.

54. Franklin Furnace, N. J. Coarse-grained magnetite collected by E. P. Kaiser from near gneiss-WL contact, 700 level, 940 shaft pillar.
Age: Pre-Cambrian. *See* Spec. No. 52.

55. Edwards Mine, Michigamme, Mich. Magnetite ore. Smithson. Inst. Coll. No. 41543.
Age: Late Middle Huronian. *See* Spec. No. 56.

56. Republic Mine, Marquette Co., Mich. Massive magnetite and hematite. Smithson. Inst. Coll. No. 41512. Separated magnetite from hematite by magnet.
Age: Late Middle Huronian, (Van Hise and Leith, 1911, p. 265). Negaunee formation, including iron formation, was considerably folded before deposition of Upper Huronian Goodrich quartzite. Goodrich quartzite contains clastic magnetite derived from Negaunee formation. Final folding of both formations occurred before deposition of Keweenawan sediments north of Marquette.

57. Spur Mt. Mine, Michigamme, Marquette Co., Mich. Magnetite ore with quartz gangue. Magnetite concentrated by magnet. Smithson. Inst. Coll. No. 41506.
Age: Late Middle Huronian. *See* Spec. No. 56.

58. Michigamme Mine, Michigamme, Marquette Co., Mich. Magnetite ore. Smithson. Inst. Coll. No. 41516.
Age: Late Middle Huronian. *See* Spec. No. 56.

59. Champion Mine, Marguette Co., Mich. Magnetite ore. Smithson. Inst. Coll. No. 41546.
Age: Late Middle Huronian. *See* Spec. No. 56.

60. Humboldt Mine, Michigamme, Marquette Co., Mich. Magnetite ore. Smithson. Inst. Coll. No. 41503.
Age: Late Middle Huronian. *See* Spec. No. 56.

61. Granite, Giant's Range, Minn. Fairly pure sample of accessory magnetite separated from granite.
Age: Algoman (?) (Leith, Lund, and Leith, 1935, p. 9).

62. Cranberry, N. Car. Coarse-grained magnetite ore. Newhouse Coll. No. 7677. About 40 per cent green silicate gangue removed by magnetic concentration.
Age: Probably pre-Cambrian.

63. Cranberry, N. Car. Coarse-grained magnetite ore. Smithson. Inst. Coll. No. 56326. Removed about 30 per cent green silicate gangue by magnetic concentration of magnetite.
Age: Probably pre-Cambrian.

64. Iron Mt., Llano Co., Texas. Massive magnetite. Smithson. Inst. Coll. No. 67291. Slightly foliated.
Age: Probably pre-Cambrian.

65. Kiruna, Sweden. Fine-grained magnetite ore with about 5 per cent apatite gangue. Newhouse Coll. No. 8762.
Age: Pre-Cambrian.

66. Kiruna, Sweden. Massive magnetite ore. Smithson. Inst. Coll. No. 89992.
Age: Pre-Cambrian.

67. Dungannon, Ont. Titaniferous magnetite. Newhouse Coll. No. 8179.
Age: Probably pre-Cambrian.

68. Narragansett, R. I. Cumberlandite. Magnetic concentration of magnetite.
Age: (?)

69. Grängesberg district, central Sweden. Coarse-grained magnetite. Newhouse Coll. No. 8561.
Age: Pre-Cambrian.

70. Falun Mine, Sweden. Fine-grained pyrite with minor disseminated quartz gangue. Sample only 70 per cent pyrite. Newhouse Coll. No. 7027.
Age: Pre-Cambrian.

71. Olivine diabase, Sudbury district, Ont. Collected by H. C. Cooke from lot 10, Concession V, McKim Twp. A magnetic concentrate was made which contained less than 40 per cent magnetite.
Age: Post-Keweenawan.

72. Norite, Sudbury district, Ont. A magnetic concentrate was made which contained less than 50 per cent magnetite.
Age: Keweenawan.

73. Worthington Mine, Sudbury district, Ont. Massive pyrrhotite and pentlandite. Newhouse Coll. No. 8918.
Age: Post-Keweenawan.

74. Frood Mine, Sudbury district, Ont. Pyrrhotite separated from massive sulphides. Newhouse Coll. No. 42.
Age: Keweenawan.

75. Caribou, Boulder Co., Colo. Coarse-grained magnetite with minor disseminated gangue. Smithson. Inst. Coll. No. 41841.
Age: Laramide.

76. Bancroft district, Ont. Coarse magnetite crystals with biotite in veinlets in syenite.
Age: Probably pre-Cambrian.

77. Craigmont district, Ont. Coarse magnetite in syenite pegmatite.
Age: Pre-Cambrian.

78. Monmouth Twp., near Tory Hill P. O., Ont. Magnetite separated from nephelite syenite.
Age: Pre-Cambrian.

79. Ketchikan district, Alaska. Magnetite separated from copper ore consisting of magnetite, chalcopyrite, and chloritic gangue.
Age: Nevadan, late Jurassic.

80. St. Charles Prospect, Chicoutimi Co., Que. Massive titaniferous magnetite.
Age: Probably pre-Cambrian.

81. Broken Hill Mine, N.S.W., Aust. Large crystals of galena. Newhouse Coll. No. 4033.
Age: Probably pre-Cambrian. (This specimen is probably a late deposition by circulating meteoric water.)

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RELIEF MAP OF NORTHERN NORTH AMERICA

Photographed from a model prepared at the Bureau of Geology and Topography, Ottawa, under the direction of D. A. Nichols. Glacial data other than existing glaciers have been added to the photograph.

Approximate outer limit of Laurentide ice sheet in Wisconsin glacial age. (Subject to considerable revision in west and north.)

Approximate outer limit of measured postglacial crustal uplift (Whittlesey hinge line in Great Lakes region; zero marine isobase in Atlantic coastal region) compiled from various sources.

Laurentide ice sheet coalescent with Cordillera Glacier complex.

Centers of radial flow:
K1, K2, K3—Keewatin centers (Tyrrell, 1898, p. 178F)
L1, L2, L3—Labradorian centers (Coleman, 1926, p. 22)
P—Patrician center (?) (Tyrrell, 1935, p. 3)

G—Gaspe center
NB—New Brunswick center
N—Newfoundland center

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GROWTH OF NORTH AMERICAN ICE SHEET DURING THE WISCONSIN AGE

BY RICHARD FOSTER FLINT

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ABSTRACT

The evidence of separate Labradorian and Keewatin centers of radial glacial flow is confined almost wholly to very late Wisconsin time, and indicates that these centers shifted their positions widely even during that short time. There is no evidence that these centers ever were the sites of independent glaciers; their importance has been overemphasized. The North American ice sheet, during the Wisconsin maximum, was recognized as a single mass and named the Laurentide ice sheet before the names Labradorian and Keewatin were applied.

The Laurentide ice sheet is believed to have originated as mountain glaciers chiefly in the conspicuous highlands of eastern Quebec, Labrador, and Baffin Island. Nourished by moist maritime air masses derived mainly from the south and southeast and moving northward and eastward, these glaciers coalesced into piedmont glaciers. By continued growth southward and westward toward the principal sources of their nourishment, the piedmonts thickened and spread, burying the highlands in which they had originated. Ultimately they formed a vast ice sheet that extended from the east coast to the Cordillera. Further eastward expansion was prevented by the deep water of the Atlantic, in which the ice broke up and floated away.

Glacial-anticyclonic winds are assumed to have been subordinate to cyclonic storms in nourishing the ice sheet. The Labradorian, Keewatin, and other centers of outflow recorded by striae were broad low domes on the surface of the ice and were caused by exceptional concentrations of snowfall. While the ice sheet was shrinking, these domes shifted position.

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INTRODUCTORY STATEMENT

Nearly half a century ago two chief centers of radial flow of widespread former glacier ice—the Keewatin and Labradorian centers—were identified through radially arranged striae. Since then the opinion has been often expressed that glacier ice at these centers originated independently and persisted throughout much of the Pleistocene. The glaciers have been referred to as the "Keewatin ice sheet" and the "Labradorian

ice sheet." Perhaps because of the supposed separate identity of these "ice sheets," they have been commonly thought of as having grown up *in situ* at these two centers, and several attempts have been made to reconstruct their growth on this presumption.

However, it appears improbable that these "centers" were ever the sites of independent glaciers (except possibly toward the end of the last deglaciation) and that either of them persisted long. It is also unlikely that glaciers grew up *in situ* by snow accumulation in these two districts, because of prevailing low altitudes and unfavorable climatic conditions.

Therefore the writer has been led to abandon the "Keewatin ice sheet"-"Labradorian ice sheet" concept and to attempt a different reconstruction of the growth of the North American glaciers. The attempted reconstruction rests on a dual basis: (1) comparison with conditions in Fennoscandia, a region that has been more thoroughly studied than northern Canada, and (2) deduction from topographic and climatologic data.

Both comparison and deduction lead to the same conclusion: that all the recognized former North American glaciers originated in mountainous highlands and that their growth was determined by climate. On this basis the glaciers can be grouped into three categories:

(1) In the Cordilleran part of the United States a variable combination of relatively high temperatures (especially in summer) and relatively small precipitation prevented the glaciers generally from growing beyond the stage of valley glaciers in the highlands.

(2) In the Canadian and locally the Alaskan parts of the Cordillera, largely because of lower temperatures, the glaciers expanded into piedmont glaciers that became generally coalescent throughout the broad belt between the Coast Ranges and the Rocky Mountains. It is doubtful whether this glacier complex was more than temporarily an ice sheet in the Greenland sense.

(3) In northeastern North America, local glaciers that formed in the mountains of the Coast of Labrador and Baffin Island, and perhaps other highland areas, grew not only into coalescent piedmont glaciers but on into a continuous ice sheet that altered the distribution of heat and precipitation, buried the mountains that had permitted the process to begin, and spread over the vast area from the Atlantic to the Cordillera, and from the Canadian Arctic to the Ohio River. Differences in snowfall on the outer parts of this ice sheet resulted in the establishment of various domelike centers of radial flow at various times; there is no indication that the centers inferred from striae are anything more than the positions occupied by the latest domes.

IDENTIFICATION AND NOMENCLATURE OF THE GLACIAL UNITS

GENERAL STATEMENT

In reconstructing the growth of the North American glaciers, we must first review the origins of the views that have been commonly held, in order to extract the definite values that they contain.

CORDILLERAN REGION

Dawson (1888, p. 349) proposed the name Cordilleran glacier for the supposed ice sheet in the Cordilleran region of Canada. T. C. Chamberlin (1895, p. 735), and later Chamberlin and Salisbury (1906, p. 329) used the designation Cordilleran Ice Sheet, a name that has been widely employed. However, more detailed studies in British Columbia have indicated that the Cordilleran region was covered, not by an ice sheet like that of Greenland, but by a complex system of coalescent valley and piedmont glaciers (*cf.* Johnston, 1926, p. 142; Leverett, 1929, p. 752; Martin, 1935, p. 8; Flint, 1937, p. 228).

REGION EAST OF THE CORDILLERA

The first specific recognition that ice had moved radially outward from the region of the Canadian Shield rather than from the Polar region was by Dawson (1886, p. 58R), who based this concept partly on the mapping by Bell (*cf.* 1884, p. 36DD-37DD) of radially arranged striae. Two years later he (Dawson, 1890, p. 162) proposed the name Laurentide glacier for the "*eastern mer de glace.*" He conceived this glacier as occupying the region between the Great Plains and the Atlantic; he thought the Great Plains region was covered by a berg-filled sea, apparently because he could not visualize an ice sheet as carrying uphill the eastern drift that lay abundantly over the Great Plains.

T. C. Chamberlin (1895, p. 725) adopted Dawson's name Laurentide glacier but rejected Dawson's marine hypothesis of origin of the Great Plains drift and applied this name to the whole inferred land-ice mass east of the Rocky Mountains.

Tyrrell (1894, p. 397) recognized evidence of radial glacial outflow from a district northwest of Hudson Bay and applied the name Keewatin glacier (Tyrrell, 1895, p. 439) to the ice mass he inferred to have centered in this district. Believing that the "centers" in the Keewatin district and in the region east of Hudson Bay (inferred from the work of Bell and others) were not contemporaneous, Tyrrell (1896, p. 812) proposed to restrict the name Laurentide glacier to the inferred ice east of Hudson Bay. However, because Dawson objected to this restriction of his term, Tyrrell (1898b, p. 150) used the name Labradorean

glacier for the glacier supposed to have "spread over northeastern North America from a center in Labrador." This name is now even less appropriate than when it was introduced, because the inferred center is no longer within the political limits of Labrador.

Following the usage proposed by Tyrrell, the designations "Labradorean" and "Keewatin" have been extensively used in professional papers and textbooks to refer to the chief centers of outflow of non-Cordilleran ice during the Wisconsin Glacial Age, while the older term Laurentide glacier, as extended by Chamberlin in 1895, has received little attention. However, this older term is certainly more in keeping with our actual knowledge of glacial conditions east of the Cordillera and has been favored by Upham (1914, p. 515), Johnston (1933, p. 16), and Leverett (1929, p. 749; 1932, p. 3; 1935, p. 7).

Many years after his earlier discoveries Tyrrell (1913, p. 197; 1914; 1935) offered evidence of radial glacier flow from a central area located between Lake Superior and Hudson Bay in the District of Patricia. He proposed the name Patrician glacier for this inferred ice mass. The separate identity of this center of radial flow has been accepted by some students of glaciation but has been questioned by others (*cf.* Leverett, 1935, p. 7).

One object of tnis paper is to show that there is justification for recognizing only two principal glacial units in the former glaciation of North America: (1) a Cordilleran glacier complex, and (2) a vast ice sheet that occupied most of the northern part of the continent east of the Cordillera, and that is here called the Laurentide ice sheet in the sense favored by T. C. Chamberlin. Further, the importance of the concept of "Labradorian ice" and "Keewatin ice" has been exaggerated, and these units were strictly limited both as to time and as to areal importance.

THE CENTERS OF GLACIAL OUTFLOW

GENERAL CONSIDERATIONS

Despite the many references to centers of radial glacier flow, the literature on this subject apparently does not include a definition of these features. From the contexts we infer that a center of radial flow is an area of unspecified dimensions from which it is inferred, on geologic evidence, that glacier ice flowed outward in all directions. The existence of such a center implies a former ice divide or ice shed, either elongate or nearly circular. The divide may have been localized by topography (examples are the British centers of outflow, all of which are coincident with highlands) or by concentration of net accumulation without topographic control (examples are the Keewatin and Labradorian centers).

The emphasis laid upon two of these centers of outflow through wide use of the names *Labradorian*[1] and *Keewatin* has distracted attention from three important facts: (1) These centers are inferred only from striae of apparently very late Pleistocene date; (2) the centers were not fixed but shifted their positions within relatively narrow time limits; (3) these were not the only centers; others existed, in part, at least, contemporaneously. If these facts are established, then the centers are less important than has been generally supposed.

LATE DATE OF THE CENTERS

The Labradorian and Keewatin centers are based on striae trending radially from ill-defined central districts. It is generally conceded that all but a specific few of the striae concerned were made very late indeed—probably very late in the Wisconsin age, for any striae recording earlier directions of flow would have been crossed or erased by striae made during the episode of latest flow. This reasoning is commonly accepted in Fennoscandia where the striae are attributed to the very latest flow because the eskers parallel them and the end moraines lie at right angles, although earlier, quite strongly divergent directions of flow are inferred from the distribution of erratics.

The existence of well-defined centers of outflow at the positions assigned to the "Keewatin ice sheet" and the "Labradorian ice sheet" not only in the earlier Wisconsin subages but in pre-Wisconsin ages as well, has been repeatedly assumed by analogy with late-Wisconsin conditions, with very little supporting evidence. It is unfortunate that drift sheets in the Mississippi basin have been identified, on a basis of lithology, as "Labradorian drift," "Patrician drift," and "Keewatin drift," whereas all that is proved by their lithologic characters is that the ice responsible for them traversed, respectively, regions southeast, south, and southwest of Hudson Bay. Thus, whereas we cannot prove that these particular centers were not functioning in earlier Wisconsin or pre-Wisconsin times, present data do not indicate that they were. That there were centers of outflow at those earlier times is not doubted, but their exact locations are not known.

SHIFTS IN POSITIONS OF THE CENTERS

Not only were these centers late, but their positions shifted. Low (1900, p. 81D) showed that the Labradorian center had three principal positions, shifting progressively northwestward through 350 to 400 miles. Tyrrell (1898a, p. 176F-179F) suggested that the Keewatin center had

[1] This spelling is preferred to the one used by Tyrrell because it conforms with the accepted usage in such names as Devonian and Permian.

occupied three successive positions involving a progressive southeasterly shift of the order of 300 miles. Low (1906, p. 232) confirmed this. Because of the nature of the evidence—striae apparently radiating outward from a poorly defined central area—the details of these inferred movements may well be questioned; but that some shifting occurred seems to be substantiated. Ice movements from the directions of the Patrician and Keewatin regions affected southeastern Manitoba at different times late in the Pleistocene (*cf.* Johnston, 1935, p. 14). Similar differences in direction of flow across the same district, though possibly involving pre-Wisconsin glaciers, have been recorded by Burwash (1935), who also inferred the possibility of other, early centers as far west as the region of Great Slave Lake.[2] In the region south of James Bay, according to Low (1906, p. 232), the striae made by southeast-flowing ("Keewatin") ice are almost obliterated by those made by west-flowing ("Labradorian") ice, thus recording a regional shift very late in the glacial occupation. The shifts in position must inevitably lead to doubt as to the importance and persistence of the two inferred ice divides that have received the greatest attention.

EXISTENCE OF OTHER CENTERS

In addition to the above centers of outflow at least three others have been identified east of the St. Lawrence: (1) Newfoundland (MacClintock and Twenhofel, 1940); (2) the highlands of northwestern New Brunswick (Alcock, 1935, p. 121, 125); and (3) the Shickshock highland on the Gaspe Peninsula (Flint, Demorest, and Washburn, 1942). Probably in all three districts the local glaciation referred to was preceded by broad, ice-sheet glaciation that originated northwest of the St. Lawrence. Further (though the evidence is scanty and indirect), the broad glaciation possibly was preceded in turn by local glaciation on these same highland sites. Possible additional glacial centers are mentioned by Coleman (1941, p. 48).

These additional centers, which are topographically controlled, together with the possible Patrician center, which is not so controlled, diminish the emphasis to be placed on the Labradorian and Keewatin centers.

Still other ice divides may have existed over the broad region now beneath Hudson Bay and beneath the emerged marine sediments of its shore. Surprisingly the probability of such centers appears never to have been considered seriously by students of the North American glaciation.

[2] Since this paper was written, the writer has seen unpublished data in the files of the Geological Survey of Canada that strongly suggest a center in northern Alberta.

CONCEPT OF A SINGLE LAURENTIDE ICE SHEET

It is thus apparent that the Keewatin and Labradorian centers of radial flow (1) were not certainly in existence before the latest fraction of the Wisconsin, (2) were not fixed in position even during that time, and (3) were accompanied by other centers identified from evidence of a similar kind.

We know fairly well the extent of Wisconsin glaciation from the Atlantic to the Cordillera, and within the vast region thus embraced the products of glacial erosion and deposition are fresh, and in the peripheral zone the later Wisconsin deposits are continuous and apparently contemporaneous, indicating that the ice throughout this whole region was then a continuous sheet. The so-called Keewatin ice sheet was necessarily continuous with the so-called Labradorian (and/or Patrician?) ice sheet in Manitoba even in very late Wisconsin time, for otherwise the later shore lines of Lake Agassiz could not have been made, as the lake would have lacked a northern shore and would have drained off to Hudson Bay (Johnston, 1935, p. 15). Of course, separation here or elsewhere may have occurred still later, as the Scandinavian glacier is believed to have separated at Ragunda.

From this we may conclude that Dawson's concept of a single Laurentide[8] ice sheet is most adequate when used in the sense understood by T. C. Chamberlin in 1895: a continuous ice sheet occupying, at its maximum, most of Canada east of the Cordillera, and having a geographical center in the general region of Hudson Bay. This concept is favored not only by negative evidence—the evidence against the outstanding importance of the Labradorian center and the Keewatin center—but also by general support from two facts:

(1) The isobases drawn thus far on marine and lacustrine strand lines in eastern and central North America, warped up by removal of the weight of the disappearing glacier ice, are not doubly concentric to the Keewatin center and the Labradorian center—they are broadly concentric to the Canadian Shield, centering in Hudson Bay itself. From inspection of the isobases alone the existence of these two centers cannot be inferred, although such inference should be possible if these two regions were the persistent sites of the thickest late-Wisconsin glacier ice.

(2) It is not the Labradorian and Keewatin centers that are still submerged today, but Hudson Bay itself, lying directly between them. The stream-dissected floor of the Bay does not suggest that this region

[8] This name is adopted because it has clear priority over all other names, and because it alone has been used in Chamberlin's sense by later writers, as noted beyond.

was an arm of the sea in immediately preglacial time. In fact the submerged valley beneath the northern part of the Bay may be the preglacial outlet of a Missouri-Nelson river (Cooke, 1930, Fig. 1). Furthermore, the Hudson Bay region appears to be still rising (Gutenberg, 1941, p. 747-750). On the east coast of the Bay Stanley (1939) found evidence of upwarping, not eastward toward the Labradorian center but westward toward the Bay. These considerations suggest that Hudson Bay was submerged because of the weight of an extensive ice sheet that was thickest over the site of Hudson Bay itself. We conclude, then, that the concept of a vast, continuous Laurentide ice sheet, in which the Labradorian, Keewatin, and other centers were of local and temporary importance, best meets the available facts.

EARLIER EXPLANATIONS OF ORIGIN AND GROWTH OF THE ICE SHEETS

EARLY HYPOTHESIS OF GROWTH *IN SITU*

Dana suggested the view commonly held toward the end of the nineteenth century regarding the localization of the former great ice sheets: that broad uplifts were the significant factor. The ice sheets were thought of as having accumulated *in situ* by snowfall induced by high elevation. Upham clearly stated this view (Wright, 1890, p. 573; Upham, 1895). Crosby (1896, p. 207, 204) said: "The Pleistocene ice-sheet was formed *in situ*, by snow accumulation, over the main part of the plain country within the glaciated area." The cause was "the general refrigeration of the climate, due chiefly, it is probable, to a marked elevation of the northern part of the continent." In the face of more modern knowledge, the uplift hypothesis was carried to an extreme by Wright (1911, p. 586) who postulated a wave of elevation that migrated from west to east across Canada, with the loci of ice sheets shifting accordingly.

The only recent suggestion of growth *in situ* was by C. E. P. Brooks (1928, p. 311) who thought that cyclonic storms precipitated much snow in the formerly ice-free region between the Cordilleran glacier complex and the Labradorian ice sheet, and that this snow accumulated to such an extent as to form an independent Keewatin mass.

Objections to this hypothesis are strong. In both the Keewatin district and the general region of the Labradorian center the climatologic conditions are unfavorable to the inception of glaciers, for two reasons: (The figures are from C. F. Brooks *et al.*, 1936.)

(1) The mean annual precipitation amounts to only about 10 inches (Keewatin) and 20-30 inches (Labradorian). These figures include an annual snowfall of about 40 inches (Keewatin) and 80 inches (Labradorian)—respectively about one-tenth and one-fifth of the annual snow-

fall in the high glacier-bearing parts of the Sierra Nevada and the Cascade and Rocky Mountains.

(2) Mean July temperatures are about 10° C. (Keewatin) and 14° C. (Labradorian). These temperatures, although not high, are clearly high enough to dispose of more snow than actually falls in these regions.

Both regions are relatively low in altitude. The Keewatin center lies less than 500 feet above sea level, and the district of the Labradorian center ranges between 500 and 1500 feet. Conditions favoring the growth of glaciers might be improved by a great increase in the altitudes of these regions, but such increase is not supported by any geologic evidence thus far reported, and the absence of deep valleys that would have resulted from uplift constitutes strong evidence against it. Alternatively, glacier growth in these regions might be favored by a widespread, perhaps world-wide, climatic change. In such a case, however, the glaciers should have formed first in the higher lands farther east and should only later have spread to the areas of the Labradorian and Keewatin centers.

Thus it appears that unless former climatic conditions in the regions of these centers, relative to the highlands, were very different from what present knowledge leads us to suppose the hypothesis of growth *in situ* should be rejected. However, Upham (1895, p. 344), one of the proponents of the growth *in situ* idea, recognized the probability that the North American ice sheet had been nourished in considerable part by moisture-bearing cyclonic storms which he thought could penetrate "100 or 200 miles inward from the margin." This concept was carried further by T. C. Chamberlin. He (Chamberlin, 1899, p. 753) stated that "neither the Labradorean nor the Keewatin centers appear to have been initiated by mountainous elevations. It was a development of glaciation on plains, or at most on plateaus." Nevertheless he emphasized (Chamberlin and Salisbury, 1906, p. 333) "the habit of the Labradorean and Keewatin sheets in expanding chiefly to windward." These are valuable concepts, and the early date of their appearance deserves attention. The concept of windward expansion has likewise been emphasized by Huntington and Visher (1922, p. 117).

HYPOTHESIS OF SEQUENTIAL DEVELOPMENT AND AN OPEN HUDSON BAY

At the time of his discovery of a center of radial flow in the Keewatin district, Tyrrell (1894, p. 397) thought of Hudson Bay as having been, at the time of glaciation, open water which furnished the moisture for nourishing the Keewatin ice.[4] Thus he realized the dilemma presented by low altitude and unfavorable climate. Soon afterward he developed a hypothesis of sequential development of the glaciers (Tyrrell, 1898a);

[4] Low (1906, p. 231) suggested that the moisture was derived from "an open Arctic sea."

1898b).[5] In this view, the Cordilleran ice was the first to form, early in the Pleistocene; in time the Keewatin ice formed, which, in turn, had shrunk away from southern Manitoba before Labradorian ice reached the longitude of Manitoba and occupied areas vacated by its predecessor. Tyrrell's maps show the presence, at least in part, of the Hudson Bay water body continuously throughout glaciation. Later, however, Tyrrell (1916, p. 16) inferred that the "Patrician glacier" had antedated the "Keewatin glacier," at least in the later part of the glaciation.

Upham (1914, p. 520-521) cited evidence against Tyrrell's concept of sequential development and emphasized the opinion, now generally held, that as Wisconsin drift is present throughout the whole peripheral zone of glaciation from New England to Alberta, the ice sheet appears to have existed contemporaneously throughout more than 2500 miles.

HYPOTHESIS OF COMPLEX MIGRATION BY WIND DRIFTING

In 1916 Enquist (1916) offered a new hypothesis of development based on the premise that although snow is precipitated chiefly on the windward side of a mountain mass, the wind drifts it leeward so that glaciers predominate on lee slopes. Although drifting undoubtedly does occur, and in some places may be significant in the nourishment of glaciers, Enquist does not adequately consider strong modifying factors such as exposure to the sun and altitude of the mountains. Having concluded that glaciers habitually form to leeward of mountain barriers, he conceived the development of the North American glaciers during the Wisconsin as follows:

Glaciation began with accumulated snowfall in the northern Cordilleran region of Canada, and leeward drifting of snow caused the glacier ice to migrate gradually east, thus creating the Keewatin ice sheet. Meanwhile barometric highs developed over "northerly regions," and the Wyville-Thomson Ridge (now submerged to a depth of about 3000 feet) connecting Greenland, Iceland, and Britain was shoaled enough to deflect the Gulf Stream southeastward. The combination of these two factors shifted the Icelandic barometric low southward so that its northern margin, characterized by northeasterly winds, lay in the latitude of Labrador. These winds, coming from the open sea, then precipitated their moisture on the Labrador mountains as snow, forming glaciers, but the northeasterly winds drifted the snow to leeward, causing the glacier ice to migrate toward the southwest. During this shift the Keewatin ice lost its continuity with the Cordilleran glaciers, and as anti-

[5] Tyrrell has studied ice movements and the development of the glaciers in central Canada over a period of more than a quarter of a century, and the nomenclature he introduced has been so widely used that special mention of his important contributions seems appropriate.

cyclonic conditions developed above it the Keewatin glacier spread toward the southwest.

Evidently Enquist recognized the unworkability of Tyrrell's Hudson Bay as a source of moisture. With the Scandinavian ice sheet as an example he postulated that the necessary moisture came from the nearest open water and was precipitated with the aid of highlands. Five main objections to his elaborate scheme are evident:

(1) It is doubtful that wind-drifted snow is the chief agent responsible for the expansion of glaciers,—although possibly locally significant this agent has not been proved to apply to existing large glaciers.

(2) To apply this principle Enquist was obliged to shift a thick glacier nearly 1500 miles eastward by blown snow precipitated chiefly on its west flank, and then to make it separate into two parts (Cordilleran and Keewatin) without adequate cause. Why anticyclonic conditions should not have developed until the central part of the mass had migrated as far east as the Keewatin district is not explained. As yet we have no geologic evidence of the flow of ice or of the transport of drift from the Rockies eastward through more than short distances; if substantiated by more detailed study this relationship is incompatible with the hypothesis of migration of glacier ice from the Cordillera to the Keewatin district.

(3) Although lacking proof of such a concept, Enquist was obliged to reverse the prevailing winds in the latitude of Labrador, through a distance of many hundred miles southwestward from the Atlantic, so that they would blow against the normal planetary wind circulation.

(4) Further, Enquist had to assume that the sea east of Labrador was open water from which moisture could be evaporated in large amount—an assumption that fails to take account of the probably low temperature of the water and of whatever sea ice was present at that time.

(5) Finally, the scheme is based on the assumption that the Keewatin and Labradorian ice masses were identifiable units from the start, that they were physically separate for a long time, and that they coalesced only later in the Wisconsin—an assumption not founded on any fact.

Enquist's views were regarded favorably by Antevs (1934; 1938) who thought the Cordilleran highlands might have been 2000 feet lower than now because of the weight of ice over them, thus facilitating the drifting of snow eastward to build up an ice sheet at the Keewatin center. Antevs derived his confirmation of Enquist's ideas partly from the stratigraphic relations of overlapping drift sheets in northern United States, such as those pointed out by Leverett. However, these relations prove nothing concerning the central areas. They prove only that these drift sheets

were deposited by continental ice flowing from different directions at different times. The relations mentioned by Antevs (1934, p. 308) agree notably with Enquist's scheme. However, as they agree equally well with the concept of a Laurentide ice sheet affected by precipitation that varied from time to time and from place to place in its southern and western parts, they do not greatly strengthen the Enquist hypothesis.

Coleman (1926, p. 18) was puzzled that ice should have developed in the Keewatin district before it developed east of Hudson Bay, but offered no alternative explanation. Later, however, he remarked (Coleman, 1941, p. 48): "If glaciation began in the east one might expect it to start with mountain glaciers radiating from the loftier peaks."

HYPOTHESIS OF HIGHLAND ORIGIN AND WINDWARD GROWTH

This hypothesis contends that the great North American ice sheet stretching from the Atlantic to the Cordillera at the height of the Wisconsin was intrusive or immigrant into the greater part of the vast region it covered. Valley glaciers formed in the mountains of northeastern North America, expanded, and on the lower lands to the west and south coalesced into piedmont glaciers that later thickened and united into a single great ice sheet. Nourished principally by snowfall from relatively warm moist air masses moving across the continent from the south and west, the ice sheet expanded in these directions, and the areas of greatest precipitation and net accumulation of glacier ice shifted outward with it, maintaining positions on the windward sides of the ice sheet. These areas, of course, were the sites of domes on the ice surface, and in response to variations in the accumulation of snow they shifted outward and laterally, during the decay of the ice sheet as well as during the period of growth. The particular centers of outflow of ice inferred from the geologic evidence are records of domes that existed late in the Wisconsin, probably some time after decay had set in.

This hypothesis is not new; yet curiously enough no one has tried to make a detailed and consistent whole of the concepts involved. The idea of high altitudes as an important factor in localizing glaciation goes back at least to Dana (1856, p. 328). The idea of nourishment by westerly winds in North America was first suggested by Upham (1895, p. 344). The two concepts of highland origin and windward growth were embodied first in a brief statement by Leverett (1916). Although he restated it in 1932 (Leverett, 1932, p. 3, 8; *see also,* 1929, p. 753), he never developed it further.

Leverett's statement was based on his study of the Wisconsin drifts of the western Great Lakes region, in which he found that "Labradorian

drift" was overlapped successively by "Patrician drift" and "Keewatin drift." Inferring a "westward wave of ice culmination" he stated that:

> "The highland of Labrador was the natural starting point of glaciation, and that because of storms coming to it from the southwest the ice-sheet grew westward to such an extent as eventually to receive more snowfall in the region south of Hudson Bay than in the Labrador district; so that there resulted a waning of ice movement from the latter district. Still later the ice-sheet grew westward into central Canada and caused the culmination of the Keewatin ice movement. . . ."

Although the facts stated by Leverett clearly show important shifts in the directions of flow in the southern part of the Wisconsin ice sheet at various times, they do not show that the radial flow over the western Great Lakes region originated in Labradorian, Patrician, or Keewatin centers. This is because drift deposits derived from rocks in the Labrador peninsula, Patricia, and Keewatin were not specifically identified. The drifts in question were derived from source areas lying in the general directions of those centers, but nearer the places of deposition. Hence these drifts prove nothing concerning movements of ice nearer the central part of the sheet.

Coleman (1941, p. 132) pointed out that the Pacific and North Atlantic oceans were inadequate sources of moisture for nourishing the Laurentide ice sheet and concluded that "the supply of moisture must have come from the Gulf of Mexico."

The hypothesis of highland origin and windward growth, its component concepts foreshadowed by Dana, Upham, and Leverett, is developed here on the basis of: (1) observed gradation of valley glaciers through piedmont glaciers into ice sheets; (2) comparison with the history of the Scandinavian ice sheet; and (3) climatologic data. Geologic information from the glaciated region, consisting chiefly of multiple sets of striae, plays but a small part in the attempted reconstruction because it is still very scanty.

GRADATION OF VALLEY GLACIERS THROUGH PIEDMONT GLACIERS INTO ICE SHEETS

Among present-day glaciers complete gradation is observed through the sequence: valley glaciers, piedmont glaciers, ice sheets. The fact of gradation is clearly stated and illustrated by Hobbs (1935, p. 7, 10-12, 19). Gradation from valley glaciers to piedmont glaciers is found today wherever ice tongues debouch from steep narrow valleys on to surfaces of gentle slope. Gradation from piedmont glaciers to ice sheets, much less common, is probably represented by the great glacier system northeast of the St. Elias Range in Alaska. Similar systems, approaching ice sheets in character, were developed from large groups of valley glaciers elsewhere in Alaska during the Wisconsin (Capps, 1931, p. 2). Kayser

(1928, p. 366) states that the Greenland ice sheet formed through the coalescence of valley glaciers into piedmont glaciers, and through merging of the latter into the continuous mass that not long ago covered Greenland even more completely than it does today.

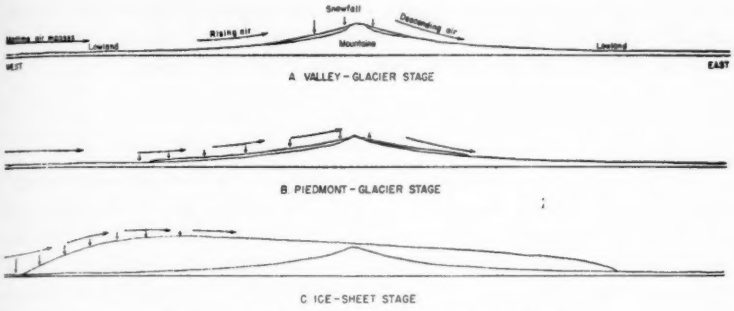

FIGURE 1.—*Development of an ideal ice sheet*

Through coalescence of valley glaciers and piedmont glaciers. Nourishment is derived from cyclonic winds moving eastward. Diagrammatic.

Such gradation is expectable and reasonable. There appears to be no basis whatever for doubting that given adequate nourishment the Laurentide ice sheet could have originated in this manner.

The ideal case would be a high mountain range with north-south axis, standing above broad lowlands and receiving precipitation from moist westerly winds (Fig. 1). With sufficient decrease in temperature, small glaciers would form high on both flanks of the mountains. They would lengthen as valley glaciers until they reached the lowlands, where they would expand as piedmont glaciers that would later coalesce to form continuous masses flanking both the windward and the leeward bases of the mountains. The locus of maximum snow accumulation, initially confined to the mountains, would be spread through a zone of increasing width owing to the effect of the expanding piedmonts, which, being both higher and colder than the same areas under preglacial conditions, would cause increased snowfall upon them. However, the windward piedmont would receive more precipitation than the leeward piedmont, and therefore glacial expansion would be more rapid on the windward side. Unless the general climatic conditions changed, the thickening and spreading of the piedmonts would continue until the ice would overtop the mountains and become a true ice sheet. Although the ice sheet would have considerable extent to leeward, its area of most active nourishment and greater thickness would be on its windward flank where it faced its prin-

cipal source of moisture (assuming the latter consisted of cyclonic winds rather than the glacial anticyclone). The ice divide, or center of radial flow, would thus have shifted some distance to windward from its original position on the mountain crest. Both the Laurentide ice sheet in North America and the Scandinavian ice sheet in Europe probably originated in this way, but in each case the ideal asymmetry deduced above was modified by deep water at the base of the mountains—on the windward base in Europe but on the leeward base in North Africa.

THE SCANDINAVIAN ICE SHEET

ORIGIN IN THE SCANDINAVIAN MOUNTAINS

The Scandinavian ice sheet (Fig. 2) formerly covered much of northern Europe. At the maximum of the last glacial age it had an area of about 1,350,000 square miles, although during at least one earlier glacial age its area was considerably greater. The significant facts are: (1) It originated in a highland, (2) its principal center of radial flow shifted away from the highland, and (3) it could have been, and probably was, nourished by snowfall from masses of moist Atlantic air.

The Scandinavian Mountains are the backbone of the Scandinavian Peninsula, reaching nearly from end to end of its 800-mile length. In one place their crest is less than 1500 feet high, but throughout considerable distances it is 4000 to 5000 feet high, and in southern Norway the highest peaks reach 8500 feet. The altitude generally increases toward the south. The mountain belt, generally regarded as a broad erosion surface upheaved in late Cenozoic time, is comparatively narrow and trends more or less transverse to the paths of the cyclonic maritime air masses that today precipitate much moisture upon their coastal flank, at a mean annual rate 30 inches in the north and more than 80 inches in the south. The precipitation is roughly proportional to the height of the mountains. Small glaciers are present in parts of this highland today.

It has long been recognized that the glaciation of northern Europe began on these mountains—in earlier glacial ages as well as in the last glacial age. The mountains harbor glaciers at present, whereas the adjacent lower regions do not; drift derived from the rocks of the mountains occurs east, south, and west of the mountain axis.

SHIFTING OF THE ICE DIVIDE

It is recognized also that the growth of the mountain glaciers to a thick coalescent ice mass led to an eastward shifting of the ice divide—the center or axis of radial outflow—from the crest of the mountains to much lower land (*cf.* Hansen, 1894, p. 130; Tanner, 1915, p. 698; 1937, p. 6-7; Enquist, 1916; Frödin, 1925). Detailed study of striae and other

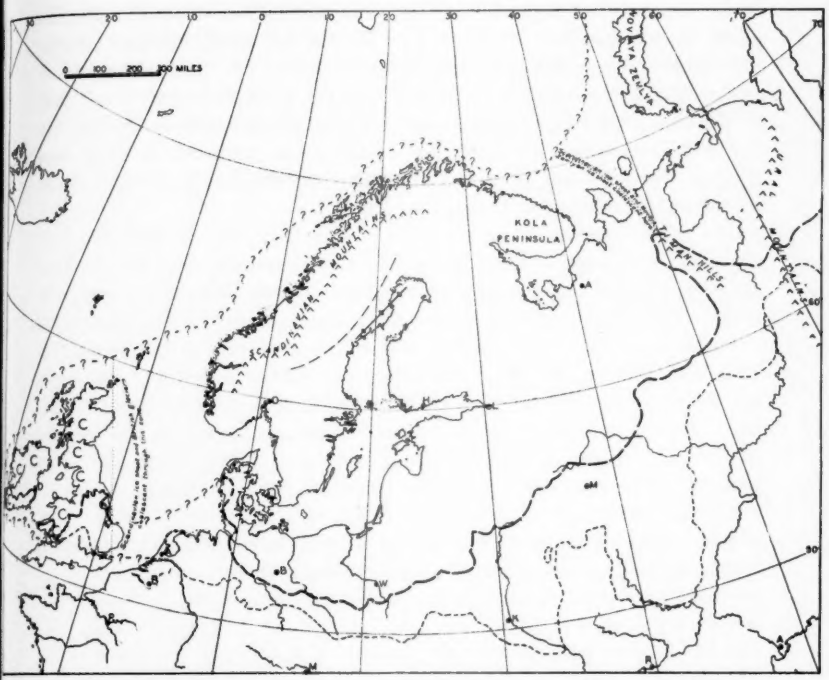

FIGURE 2.—*Northern Europe showing relations of the Scandinavian ice sheet*

——— Approximate outer limit of Scandinavian Ice Sheet during the last glacial age (Younger Drift in British Islands, Warthen drift in western Continental Europe, Würm drift in Russia).

——— Approximate outer limit of Scandinavian Ice Sheet during its maximum extent at any time.

-?-?-?- Possible outer limits of Scandinavian Ice Sheet and British glaciers in areas now submerged.

——— Most easterly position of ice divide as determined from glacial features (Tanner, 1915, p. 141; Frödin, 1925).

———— Axis of area of greatest postglacial uplift (Niskanen, 1939).

C Local highland centers of radial flow on the British Islands.

Base from map published by Prussian Geological Survey, 1925, and Atlas of the U.S.S.R., 1937. Glacial data compiled from various sources.

erosional features and the distribution of erratics has established that the shift occurred through at least 100 miles, and possibly more. There is good evidence that the ice divide occupied two or three distinct positions east of the mountains at different times (Frödin, 1925), but like the North American centers these date from very late in the last glacial age. The fact that the latest flow recorded is radial flow from an axis east of the mountains rather than valley-glacier flow eastward from the moun-

cipal source of moisture (assuming the latter consisted of cyclonic winds rather than the glacial anticyclone). The ice divide, or center of radial flow, would thus have shifted some distance to windward from its original position on the mountain crest. Both the Laurentide ice sheet in North America and the Scandinavian ice sheet in Europe probably originated in this way, but in each case the ideal asymmetry deduced above was modified by deep water at the base of the mountains—on the windward base in Europe but on the leeward base in North Africa.

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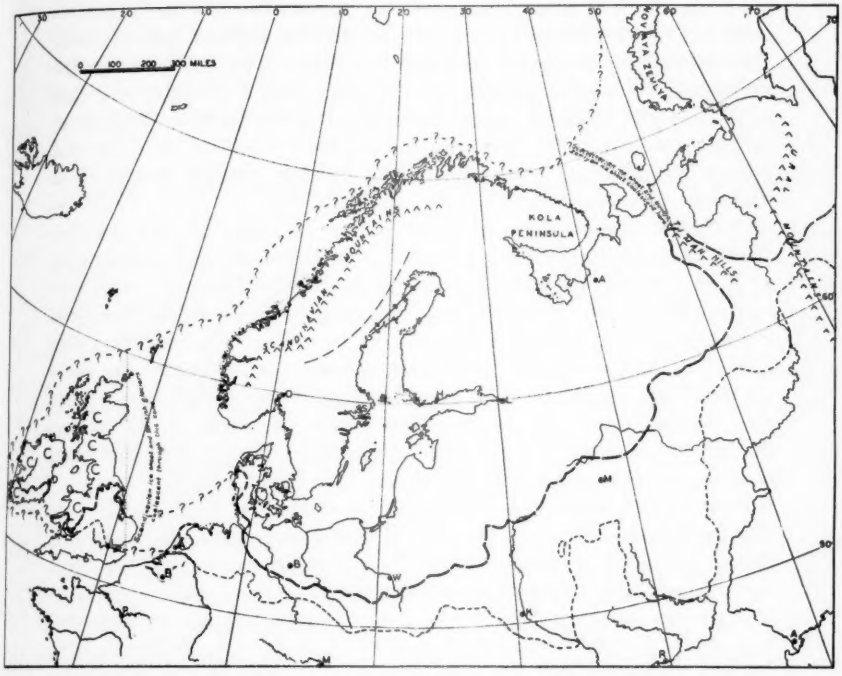
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tains, suggests that deglaciation was so rapid that sizable valley glaciers could not develop before the climate became too mild to support them.

Evidence from Denmark confirms this shift. The areal distribution and stratigraphic relations of erratic boulders in that country show that in the last glacial age (designated in Denmark as Third or Würm) ice invaded Denmark first from the north—from southern Norway and western Sweden—and later from eastern Sweden and the Baltic (S. A. Anderson, personal communication, 1937).

The same relations have been proved for the drifts of both the first (Mindel) and second (Riss) glacial ages in Denmark, with the striking addition that the east Swedish-Baltic element in the Riss drift sheet is in turn overlain by west Swedish-Norwegian drift; after the Baltic ice retired from Denmark its place was occupied by ice centering in the mountains (Madsen, 1928, p. 98; 88). The Danish record is essentially the same for three distinct glacial ages, demonstrating that the Scandinavian ice sheet formed repeatedly in the same manner, although at the end of the last glacial age it may have disappeared so quickly that flow from mountain glaciers could not re-establish itself over Denmark.

Further confirmation of eastward shift of the ice divide is the fact that the isobases drawn on upwarped lake and marine strand lines formed during and after the uncovering of Fennoscandia by the last ice sheet are concentric not toward the mountains but toward an area in Ångermanland, on the west coast of the Gulf of Bothnia, roughly between 63° and 64° N. Lat. and between 17° and 19° E. Long. (*cf.* Niskanen, 1939, Fig. 2), suggesting that the thickest ice lay far east of the mountains. Furthermore, the center is elongate parallel with the mountains, suggesting a genetic relationship between these two features.

The shifting of the ice divide could have occurred in this way (Fig 3): The chief locus of snowfall was on the windward (western) side of the mountains, as it is today, but some snowfall—augmented perhaps by snow wind-drifted in the manner emphasized by Enquist (1916)—accumulated also on the leeward (eastern) side. Although the total accumulation of snow and therefore glacier ice probably was greater on the western side than on the eastern, nevertheless the removal of ice by glacier flow was far greater on the western side. This is because the average slope is steeper and, more important, because the glacier ice reached the sea within a short distance, broke off in deep water, and floated away (*cf.* Hansen, 1894, p. 130). Hence the ice that flowed westward had a short and swift journey on a steep slope. However, the ice that flowed eastward had a slower journey on a longer and gentler slope and was not subject to calving at its termini. It may have been favored to some extent, also, by wind drifting of snow toward the east. Thus, despite pos-

sibly greater accumulation on its western side, the ice drained away as fast as it accumulated, whereas on its eastern side, despite less accumulation, drainage was slower and terminal losses were less. In consequence the east-flowing valley glaciers grew and coalesced on the Bothnian lowlands into a vast piedmont apron that spread out, thickened, and, with

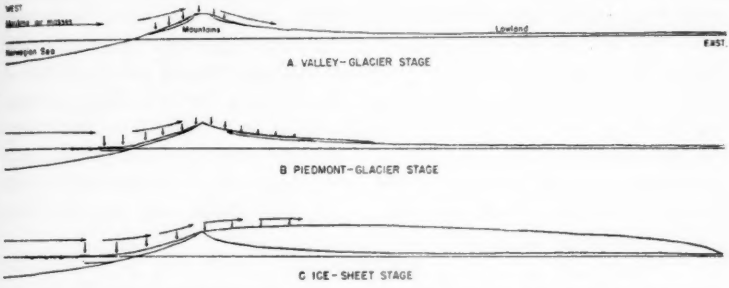

FIGURE 3.—*Development of an ice sheet such as the Scandinavian*
Relations shown are diagrammatic; vertical scale much exaggerated.

continued accumulation, "backed up" into the valleys until its summit covered the lower and perhaps also much of the higher part of the mountain crest. The ice on the east was now flowing under pressure control (Demorest, 1942, p. 38) and was in fact a true ice sheet, but the ice on the west continued to flow downward under strong drainage control. A net accumulation, whether caused by direct snowfall or wind drifting or both, greater east of the mountain crest than west of it, in time would produce an eastward shift in the center of radial pressure-controlled flow, despite a probably greater total precipitation on the west side.

The warm, moist, subtropical air masses moving from the southwest were fully competent to nourish this part of the ice sheet, provided that the temperature was lowered sufficiently to increase significantly the proportion of precipitation that fell as snow (as compared with the present proportion).

The distance of known eastward shift of the ice divide—100 miles—added to the 75 miles or so from the mountain crest to the sea may represent the distance through which maritime air masses moving eastward were able to flow upward upon the steep flank of this ice sheet and precipitate snow upon it effectively.

ASYMMETRY

The Scandinavian ice sheet was strikingly asymmetric. During the last glacial age it had a maximum southeast radius of about 800 miles,

reaching nearly to Moscow, and its predecessor, in the Riss glacial age, had a radius in the same direction of about 1300 miles. In contrast, its west and northwest radii were never more than 200 miles.

The asymmetry resulted largely from the fact that the only direction in which the ice sheet could expand far was toward the southeast. Expansion to the northeast was blocked by glacier ice flowing southwestward from the Urals and Novaya Zemlya, and to the west and north it was blocked by the deep waters of the Norwegian Sea and the Arctic Ocean, in which it must have broken up and floated away within a short distance off the present shore. To be sure, the ice sheet reached southwest to the British Isles, a distance of 400 miles from the Norwegian mountains, but this radius, twice as long as those to the west and north, was undoubtedly made possible by the fact that the North Sea is very shallow, and that during the last glacial age its floor was partly or wholly emerged.

NOURISHMENT

Cyclonic air masses feed the Scandinavian glaciers now and undoubtedly did so during the last glacial age, although we can hardly doubt that during the height of that age anticyclonic conditions were present and that they interfered, perhaps to a considerable extent, with the planetary winds. Cyclones cross the Greenland ice sheet now and precipitate snow upon it, though probably they did so with greater difficulty when that ice sheet was more massive and when, presumably, anticyclones were more active.

Probably the greatest southeastward extension of the Scandinavian ice sheet was made possible by snowfall from cyclonic air masses that skirted the polar front established by the southern border of the nascent glacier. The paths of the present-day storm tracks plotted by Birkeland and Föyn (1932, p. L2) and Alt (1932, p. M15) across Europe, together with any reasonable modifications imposed by the presence of the ice sheet itself, are well adapted to nourishment of the southern and southeastern part of the glacier. Because there are no high and continuous mountains in western Europe south of Norway, there was not much precipitation of atmospheric moisture until the cyclones encountered the southern margin of the ice itself. Here effective snowfall should have occurred, aiding substantially in the extension of the ice sheet. The combination of radial flow from the Scandinavian center (now migrating eastward from the mountains) and the "helper process" of precipitation from maritime air masses traveling eastward along the southern border of the ice, and possibly extending inward through a considerable distance from the border, seem to explain adequately the great southeastward extent of the ice sheet.

Strong lobation of the southeast margin of the ice sheet (Fig. 2) even at its maximum, under the influence of very moderate topographic control, suggests that in this sector the ice was conspicuously thin through hundreds of miles inward from its margin. This was probably because precipitation from the maritime air masses was diminishing rapidly toward the southeast, so that ablation losses on this part of the ice sheet had to be made good largely by flow from the northwest rather than largely by direct snowfall. Thus the Timan Hills, standing only a few hundred feet above the surrounding lowlands, produced a re-entrant nearly 300 miles deep in the margin of the ice sheet at the maximum of the last glacial age. Similarly, at the maximum of the Riss glacial age, the hills between the Dnieper and the Don, standing only 250 to 500 feet above the surrounding country, caused another re-entrant nearly 300 miles deep.

Thus it appears that the Scandinavian ice sheet could have been nourished from two sides. Probably its southeasterly termination was fixed by diminished precipitation and increased summer temperatures; on the northeast for a time it was coalescent with the thin ice sheet spreading outward from the highland of Novaya Zemlya, and as the latter waned the Scandinavian ice sheet spread over the region vacated (Ramsay, 1912).

Whether the ice sheet could have been nourished on its northern side also is an open question. An important path of relatively warm moist air masses today skirts the northern Scandinavian coast to and beyond the Kola Peninsula and may be effective in nourishing the present-day glaciers of Novaya Zemlya. Nevertheless, because the presence of a Scandinavian ice sheet may have blocked off this path of maritime-air movement, we cannot be sure that this source of nourishment was available when the Scandinavian ice sheet was extensive. However, the fact that the Scandinavian and Novaya Zemlya ice sheets were at least for a time co-existent suggests that the former was not wholly effective in barring nourishment from the latter.

Although there were several subsidiary centers of radial flow (all of them on highlands) in the British Isles, these were independent of the Scandinavian ice sheet. All these highlands receive heavy precipitation today. When the margin of the great ice sheet lay along the east coast of Britain the polar-front zone was pushed southwestward, temperatures in the peripheral region were reduced, snowfall was increased, and the British highlands became the loci of sizable glaciers.

Perhaps we should not expect to find subsidiary centers of radial outflow as integral parts of the Scandinavian ice sheet, in the eastern part of the region covered by it, if flow from Scandinavia rather than

upbuilding by marginal snowfall was the principal factor in bringing the ice sheet over this eastern region. However, it does not seem possible, on a basis of existing knowledge, to evaluate the quantitative importance of these factors.

THE LAURENTIDE ICE SHEET

INTRODUCTION

The preceding section emphasizes two important points that are generally accepted by European glacial geologists: (1) The Scandinavian ice sheet originated as glaciers in the Scandinavian Mountains; (2) during the growth of that great glacier, the ice divide came to occupy a position conspicuously east of the mountains. With these points in mind, we may now turn to North America east of the Cordillera, in an attempt to discover to what extent conditions here may have been similar.

The concept to be developed is that the Laurentide ice sheet, embracing both the "Labradorian ice" and the "Keewatin ice," was immigrant into most of the region it covered when at its maximum; that it originated as small valley glaciers in certain mountainous highlands; that these glaciers expanded into piedmont glaciers on the lower lands to the west and south; that the piedmonts thickened and coalesced to form an ice sheet; and that nourished by moisture-bearing winds from the south and west, the ice sheet expanded until it covered more than 3,500,000 square miles—nearly three times the area covered by the Scandinavian ice sheet.

The attempted reconstruction refers only to the Wisconsin, although it seems likely that if it should prove to be valid for that age it would also hold for earlier ages. The reconstruction assumes an initial condition of complete or almost complete deglaciation.

THE HIGHLANDS

Northeastern North America (exclusive of Greenland and Ellesmere Island, which will be considered later) has three principal highland areas in the region north of Lat. 50°: the mountains of Baffin Island, the mountains of the Coast of Labrador, and the highlands of eastern Quebec (Pl. 1). The mountains of Baffin Island are more than 500 miles long, and have a somewhat sinuous northwest-southeast trend, occupying the central and northern parts of the island. They are high throughout this length, some summits reaching an altitude of more than 6000 feet. In the southern part of the island are extensive though lower highlands, nearly all parts of which are less than 3000 feet in altitude.

The mountains of the Coast of Labrador lie between 57° and 60° N. Lat., with a total length of between 200 and 250 miles. They consist

of several fairly distinct mountain groups or ranges together forming a belt about 50 miles wide along the coast. Considerable areas reach altitudes of more than 3000 feet, and the highest peaks stand nearly 5000 feet above sea level.

The highlands of eastern Quebec lie in the southern part of the peninsula bounded by Hudson Bay, Hudson Strait, the Atlantic, and the St. Lawrence. Their highest part forms the divide between the St. Lawrence River and Hamilton River, and the boundary between Labrador and the Province of Quebec. They are an irregular mass some 150 miles in diameter, and their highest parts are more than 3000 feet above sea level.

Cooke (1929, p. 119) has given evidence that the highlands of Quebec and the Coast of Labrador were warped up to their present positions in late Pliocene time. There appears to be little information as to the date of uplift of the mountains of Baffin Island.

CLIMATIC CONDITIONS

Data on the climate of northeastern Canada are very scanty; hence even the generalized statements made here are subject to future revision. The whole region embracing the highlands lies within the mean January isotherm of −15°C, and the summers are cool and short. Mean July temperatures are believed to be 4°C on Ellesmere Island, 6°C on Baffin Island, 10°C on the Coast of Labrador, and 14°C in extreme eastern Quebec (C. F. Brooks *et al.*, 1936, Map 8). Connor (1938, p. J351) points out that northeastern Canada is "practically summerless" because the rotation of the earth deflects cold northern winds and marine currents toward it and at the same time deflects warm southern winds and the Gulf Stream away, and he adds that the very cool summers in this region favored the great glaciers.

At present there are no glaciers in eastern Quebec, and those in the mountains of Labrador, although fairly numerous, are small. On the other hand, Baffin Island, where summer temperatures are lower, has sizable valley glaciers and plateau ice caps.

On Baffin Island the mean annual precipitation is believed to range from 10 inches at the northwest to 20 inches at the southeast. These figures are based on inadequate data (Sverdrup *et al.*, 1935) and may be higher at higher altitudes. On the Coast of Labrador and in eastern Quebec the mean annual precipitation is about 30 inches (C. F. Brooks *et al.*, 1936, Map 14). These figures are small, largely because the prevailing winds come to the northeastern North American highlands from the west and southwest across a broad continental region.

This broad continental lowland on the windward side of the highland areas constitutes a topographic-climatologic relationship which is the reverse of that in northern Europe, where the broad lowland is leeward of the mountains. In Europe, therefore, a considerable part of the precipitation that formed glacier ice on the windward slope of the growing Scandinavian ice sheet flowed rapidly westward into the sea, calved off, and floated away, and hence was not available for extending the ice sheet in its main direction of growth, which was necessarily across the lowland to leeward. In northeastern North America, on the other hand, if we grant the initial growth of glaciers in the coastal highlands, glacier expansion was favored by the fact that the windward side, the locus of chief precipitation, is the continental side. In consequence there were no losses by calving on this side, and all the precipitation here was available for glacier expansion—less, of course, the normal losses by wastage other than calving.

When we compare the 10 to 30 inches (sea-level figures) of annual precipitation on northeastern North America with the 30 to 80 inches on the Scandinavian Mountains, it might seem at first that the Laurentide ice sheet either should have been smaller than the Scandinavian or should have grown more slowly. However, this was not true (1) because of the great loss to the Scandinavian ice sheet by calving on its windward side, and (2) because the 10 to 30 inches of rainfall now precipitated annually on the North American highlands are by no means all that could be precipitated in this region under glacial conditions. Meteorologists (*cf.* Holzman, 1937) now recognize that over eastern North America the atmosphere contains abundant moisture; that lack of precipitation is due not to lack of atmospheric moisture but to lack of the conditions necessary for precipitating it; and that highlands exert a much greater influence upon precipitation than does nearness to the oceanic sources of moisture. The southeasterly, southerly, and southwesterly winds in northeastern North America bring maritime air masses from the Gulf of Mexico, from the Atlantic, and even from the Pacific Ocean, and apparently these masses still contain moisture for precipitation even after they have traveled 2000 miles from the Gulf to Quebec or 3000 miles from the Pacific Coast. In consequence, once the Laurentide ice sheet was well started, its high altitude and low temperature should have provided polar-front conditions capable of extracting from the cyclonic air masses more moisture than is now precipitated on northeastern North America.

It appears, therefore, that so far as the significant factors can be assessed qualitatively (we lack the data for assessing them quantitatively)

an ice sheet could be derived from the highlands of northeastern North America provided the accumulation of snowfall could be started.

GROWTH OF VALLEY GLACIERS

Small glaciers are present today on the Coast of Labrador, larger ones occur on Baffin Island, and still larger glaciers exist on Ellesmere Island. It seems necessary to begin by assuming a temperature—at least a summer temperature—lower than that which now exists, in order to reduce the altitude of the snow line and to cause the accumulation of snow and the expansion of glaciers in the critical highlands. It is not proposed to enter here into the question of the basic cause of the important changes of climate during the Pleistocene. The assumption of lowered temperature, whatever the cause, appears to be essential to the inception of the Laurentide ice sheet.

Paschinger (1923) has clearly indicated the importance of the relationship between the vertical distribution of snowfall on a highland, and the snow line. Where the highland has sufficient altitude, the mean annual snowfall increases upward to a maximum value and then, at higher altitudes, diminishes. Under present climatic conditions the snow line, determined chiefly by summer temperatures, normally lies higher than the level of maximum snowfall. Therefore, if the snow line descended it would successively occupy positions at which there is more snow. This would increase the persistence of fallen snow into the summer season. If the snow line should descend as far as the level of maximum snowfall, the development of glaciers would be increasingly favored.

This "self-extending" factor is accompanied and followed by others. Following Ångstrom, C. E. P. Brooks (1928, p. 128) believes that whereas about 20 per cent of the solar radiation received per unit area by a land surface not covered by ice or snow is reflected back into space, about 80 per cent of that received by a glacier or snow-covered area is lost by reflection, and that the lost portion would be sufficient, in temperate latitudes, to melt more than 30 feet of ice over the unit area annually. Brooks inferred that this would be a large factor in the expansion of glaciers. Not only would the loss of heat have diminished melting and evaporation of ice and snow, but by decreasing the temperature of the highland it would have induced greater precipitation. When the glaciers had become extensive enough to exert a broad regional influence, their presence would have increased appreciably the thermal gradient between low latitudes and high, and would thereby increase wind velocities, atmospheric turbulence, lateral mixing of air masses, and the rate of evaporation and precipitation. Once the glaciers were in existence, this could only contribute to the rapidity of their growth.

PIEDMONT GLACIERS: ICE SHEETS

With continued growth (Fig. 4), the valley glaciers descending the eastern slopes of Baffin Island calved into Baffin Bay and Davis Strait. Those descending the western slopes coalesced to form piedmont glaciers on the lowlands beyond, for it is unlikely that the very shallow Foxe

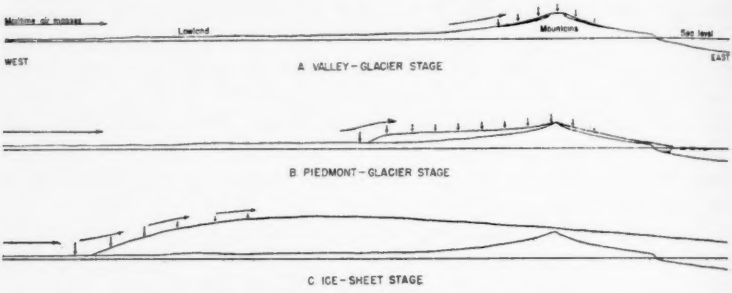

FIGURE 4.—*Development of an ice sheet such as the Laurentide*
Diagrammatic, with exaggerated vertical scale.

Basin was at that time submerged. This statement is based on the assumption that crustal recovery in that region is not yet complete, that when it is complete there will be little or no submergence of the Foxe Basin and Hudson Bay, and that early in the Wisconsin the absence of glacial loading provided conditions similar to those expectable when present recovery is complete. The expanding piedmont ice began to compete with the mountains back of it for a share of the available snowfall. As the ice thickened and spread, chilling the air above and beyond it and creating a new secondary topographic barrier to atmospheric movement, it must have received increasing snowfall at the expense of the mountains. The broadening and thickening ice sheet would have buried the mountains, eliminating them as a topographic and meteorologic factor.

The fundamental difference between the mutual relations of wind direction, highland, plain, and sea here, as compared with Scandinavia, is that here the vast continental area over which the ice could spread lay to windward of the highlands. Little snowfall here was lost by calving of ice into the sea; on the contrary the snowfall remained to build up an ice sheet that not only constituted a more formidable topographic barrier to moving air masses than the original mountains had been, but also strengthened the existing atmospheric polar front and pushed it southward.

The conversion from gravity flow to extrusion flow (Demorest, 1942)

in the coalescing piedmont extensions of the glaciers probably was accomplished when they had crept short distances out beyond the highlands. Later, when the areas of extrusion flow had backed up to the crest of the highlands, the whole glacier passed from drainage control to pressure control and became a full-fledged ice sheet.

Probably before this time small glaciers had begun to form in the highlands of the Coast of Labrador. Not long after, glaciers may have developed in the highlands of eastern Quebec. Spreading outward, they would have coalesced, creating an ice sheet over the low areas east of Hudson Bay. At the same time this ice sheet would have expanded northward, ultimately coalescing on its northwestern flank with the sheet spreading outward from Baffin Island. Thus the sheets on the two sides of Hudson Strait would have been welded into a continuous whole, and the Laurentide ice sheet proper could be said to have come into existence. Hudson Strait itself, with a maximum present depth of 1300 feet, and with probably a smaller depth at that time, could not have been an important obstacle. Like the trough of comparable depth off southern Norway, which failed to prevent the Scandinavian ice sheet from reaching the east coast of Britain, it would have filled up with glacier ice, grounded firmly on the bottom. Both troughs, however, probably were important paths of seaward extrusion flow in the basal ice.

SOURCE OF SNOWFALL

By the time the ice sheet had attained this considerable size, anticyclonic conditions must have been established above it, conspicuously modifying the regional system of air circulation. Whether an anticyclone is capable of nourishing and expanding a large ice sheet unaided is a question not yet conclusively answered. The problem of anticyclonic versus cyclonic nourishment of the Greenland ice sheet is briefly discussed by Demorest (1941), who doubts the quantitative ability of anticyclonic winds to nourish that ice sheet. Also, low-pressure air masses cross the Greenland ice sheet and furnish snowfall to the interior (Loewe, 1935, p. 350). Thus it seems best at present to assume the nourishment of large ice sheets chiefly by cyclonic air masses. In any case the question of ice-sheet origin is not greatly concerned with the question of anticyclonic nourishment, for anticyclonic circulation could hardly have come into existence until the ice sheet had developed to a considerable size.

On the cyclonic assumption the chief nourishment of the growing Laurentide ice sheet would have come from warm, moist maritime air masses traveling northerly and easterly from the Gulf, the Atlantic, and the Pacific. Their present-day paths are shown in plottings, admittedly

incomplete, by Connor (1938, Figs. 53-54; *see also* Ward and Brooks, 1936, p. J124). These warm, moist masses would have risen up over the cold margin of the growing ice sheet and over wedges of cold air extending southward from it, with resulting heavy snowfall that may have been effective throughout a wide belt on the glacier. The ice sheet, being both higher and colder than anything in this region today, not only increased the outflow of cold polar air from the northeastern part of the continent but also constituted a topographic barrier that caused precipitation of moisture from the maritime air masses as they encountered it. At the same time the air masses from all these sources (Tropical-Gulf, Tropical-Atlantic, Tropical-Pacific, and even Polar-Pacific) would have had a tendency to be deflected generally eastward along the broad southwestern and southern marginal zone of the ice sheet, with resultant further snowfall along this path. In consequence of this, and of the drifting of snow radially outward with glacial-anticyclonic winds blowing from the central part of the ice sheet, the net accumulation in this broad zone probably was greater than the net accumulation in the central part of the ice sheet. The southwestern and southern parts of the ice sheet would then have been thicker than the central, northern, and northeastern parts, resulting in differential pressures causing extrusion flow radially away from the thickest ice, maintaining equilibrium (Demorest, 1942, p. 39) and keeping the broad expanse of the ice sheet nearly flat.

Polar-continental air masses, sweeping over northern North America from the northwest, are not responsible for much precipitation in this region today. Possibly this air provided somewhat more precipitation on the northwestern part of the Laurentide ice sheet than it does today, because of the high cold border of the former glacier. However, polar-continental precipitation could hardly have been great there, as indicated by these facts: The Brooks Range in northern Alaska trends east-west, oblique or transverse to winds from the Arctic Ocean. Yet not only does its northern flank receive only 6 to 10 inches of precipitation annually at present, but during the Wisconsin the snowfall was sufficent only to nourish semicoalescent valley glaciers; the Arctic lowlands north of the mountains were not glaciated at all (Capps, 1931, Pl. 2). Probably for the same reason the extensive glaciers of Ellesmere Island are confined to the southern and central parts of the island, while the northern part is nearly ice-free (Smith, 1931, p. 65).

GREENLAND AND THE ARCTIC ARCHIPELAGO

We have no direct evidence as to the time relation of glacier growth in Greenland to that on Baffin Island and the mainland. Because of

the high latitude and high mountains of Greenland, the inception of glaciers in that region might have been somewhat earlier than, or at least contemporaneous with, the growth of glaciers on Baffin Island. The mountains are massive highlands along both coasts, the higher summits reaching 6000 feet on the west coast and 10,000 feet on the east coast. Whether mountains are present in the interior, beneath the ice sheet, is not known.

Following the inferred pattern of events in Scandinavia, Kayser (1928, p. 366) says of the origin of the Greenland ice sheet:

> ". . . hardly any one will doubt that the inland ice was originally a formation of great heights, neves arising in the higher mountain regions when the climate became sufficiently cold; afterwards they increased in size and, in the form of glaciers, extended still farther into the valleys, till these were filled with ice, and the former numerous isolated neves grew together into a few larger ones and, finally, into a single ice field, above which only isolated peaks projected, until also they, in the central parts, were buried under the constantly increasing ice cover."

Valley glaciers would have calved into Baffin Bay and Davis Strait from both Greenland and Baffin Island, as they do today, but the growth of ever-thickening piedmont ice and ice sheets entering this water body may have completely filled the basin of Baffin Bay, which has extreme present depths of 6000 feet. Possibly glacier ice extended across Baffin Bay, resting on the bottom, discharging, with increments from Hudson Strait and Labrador, into the Atlantic, and welding the Greenland and mainland ice sheets into a single unit which, at its maximum, would have extended from East Greenland to the Rocky Mountains.

A partial check on this deduction should be furnished by a study of any emerged postglacial marine strand lines in Labrador, Baffin Island, and southern Greenland. The trends of the isobases drawn on these strand lines should give some indication of whether thick ice occupied the basin of Baffin Bay as well as the interior of Greenland and the region southwest of Baffin Island.

In any case the development of an ice sheet upon and southwest of Baffin Island may have reduced the Greenland ice because the windward position of the former should have put it in a position to preempt much of the nourishment of the latter. On the other hand, the Greenland glacier may have been sufficiently well nourished by Atlantic maritime air masses to have been unaffected by the growth of the Laurentide ice sheet. However, when the Laurentide ice sheet itself was at its maximum, and its most active part was its southwestern and southern peripheral region, probably its central and northeastern parts correspondingly suffered from want of nourishment and were reduced as compared with their earlier condition.

The only other islands in the Arctic Archipelago with considerable height are Ellesmere Island and its neighbor, Axel Heiberg Island. On Ellesmere Island the heights are believed to reach more than 6000 feet; they are as high, though not so extensive, as those of Baffin Island. Glaciation may have begun on Ellesmere Island as early as on Greenland. Most of the rest of the Archipelago lies below 1000 feet, and the water bodies are predominantly shallow. Washburn (1942) has evidence that most or all of the islands may have been glaciated, although the data are insufficient to permit inference as to generalized directions of flow. Reasons have been stated for the belief that Polar-continental air masses were not important snow-bringers even during the Wisconsin. If they were not, then it seems likely that the ice in the Archipelago was chiefly immigrant by glacier flow from the east and south where the snowfall was greater, as was apparently true of the southeastern part of the Scandinavian ice sheet.

THE CORDILLERAN GLACIER COMPLEX AND THE LAURENTIDE ICE SHEET

In one sense the Cordilleran glacier complex was the logical North American counterpart of the Scandinavian ice sheet in Europe, occupying as it did a lee coast marked by high mountains and with deep water immediately offshore. Yet this glacier system was confined chiefly to the region between the Coast Ranges and the Rocky Mountains in Canada and covered an area hardly one-quarter the size of that covered by the Laurentide ice sheet. Why did it not grow far to leeward as did the ice sheet in Europe?

Where northwestern Europe has only one mountain chain, western Canada has two: The Coast Ranges and the Rockies. These two chains, together with the intervening highlands, produce today a rain shadow much greater than that produced by the Scandinavian Mountains. Furthermore during the last glacial age the effectiveness of the rain shadow in Europe was minimized by warm air masses that moved along the southern flank of the ice sheet, where no high mountains exist. In western North America the high mountains continue far south of the region of extensive Cordilleran glaciation. The Coast Ranges, Cascades, and Rockies south of the 49th Parallel create strong rain shadows. Hence the growth of Cordilleran glaciers was limited by the available snow accumulation in the vicinity of the mountains themselves. Although these glaciers swamped the country between the Coast Ranges and the Rocky Mountains in Canada, they appear to have reached the status of an ice sheet only temporarily if at all. The evidence indicates that the glaciers descending the east slope of the Rockies extended only a short distance east of the mountains. We may infer that these

glaciers were prevented from receiving abundant nourishment as the westerly winds yielded snow to the much more massive glaciers farther west and then, descending the east slope of the Rockies, were adiabatically warmed and precipitated little on the lee slopes. However, as the

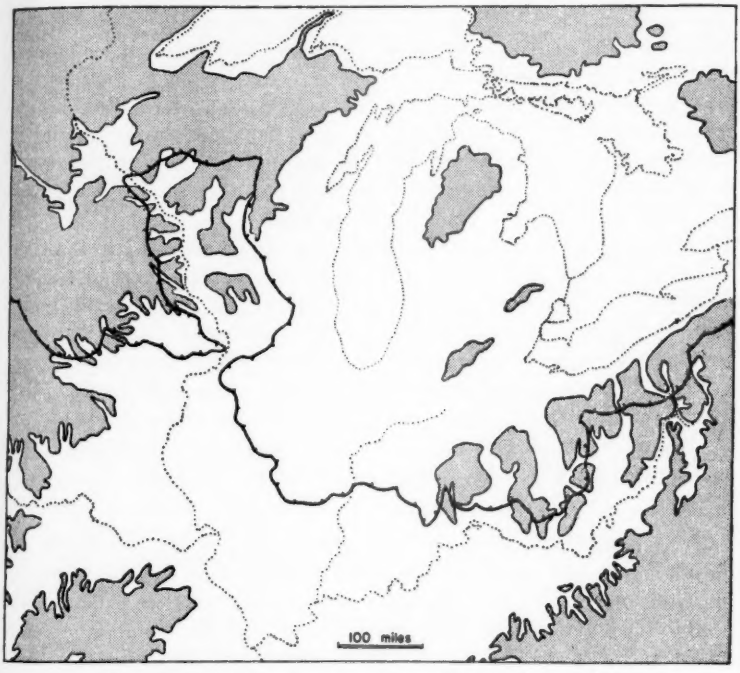

FIGURE 5.—*Sketch map of part of central North America*

Showing relation of the Laurentide ice sheet to topography during the Wisconsin glacial age. Areas with altitude greater than 1000 feet are stippled. Inferred margin of the ice sheet is shown by hatched thick line. Major drainage features (Mississippi, Ohio, Wabash, and Missouri rivers, and Great Lakes) are shown by dotted lines. Base and altitude data generalized from maps published by U. S. Geol. Survey, and Topographical Survey of Canada.

margin of the spreading Laurentide ice sheet reached the vicinity of the Rockies additional moisture was precipitated as snow upon the coalescent Laurentide and Cordilleran ice.

However, even when the Laurentide ice sheet was at its maximum the rain shadow of the Rocky Mountains made itself felt. The border of the Wisconsin drift as it is traced westward from Illinois to the mountains swings strongly north through more than 9 degrees of latitude in less than 1500 miles. Probably a large factor in this swing is the rain-

shadow effect of the mountains (noted by Huntington and Visher, 1922, p. 121), which resulted in decreasing precipitation with increasing proximity to the mountains, because the precipitation in this region is derived chiefly from winds crossing the mountains from the direction of the Pacific.

Another factor in the northwest-southeast trend of the margin of the Laurentide ice sheet between the Mississippi and the Rockies was the effect of nourishment by Tropical-Atlantic and Tropical-Gulf air masses which commonly followed the Mississippi Valley region in their northward and eastward paths. (*Cf.* Connor, 1938, Fig. 53.) With heavy precipitation from this source the ice sheet expanded farther south in the Mississippi Valley region than in the adjacent sectors less commonly visited by air masses from the Gulf, and thus produced a broad southward bulge in the margin of the ice sheet.

A third factor was topographic; the Mississippi Valley is also a region of low altitude (Fig. 5). Over this region the marginal part of the ice sheet would have been thicker than over either the Appalachian highlands to the east or the higher land to the west and would therefore have flowed more rapidly, producing a broad salient in the ice-sheet margin. Conversely, the steady increase in altitude between the Mississippi River and the Rocky Mountains, resulting in a difference in altitude of several thousand feet, played a part in the northerly component of trend of the ice margin across the Great Plains.

CONDITIONS AT MAXIMUM

If the expanding Laurentide ice sheet were nourished chiefly by air masses moving northward, eastward, and northeastward from the Atlantic, the Gulf, and the Pacific, the most active part of the ice should have been a broad marginal zone where relatively large accumulation induced relatively rapid flow. With continued nourishment predominantly over its southern and western parts, the ice sheet expanded westward until the long uphill slope of the Great Plains of Canada ended in the Rocky Mountain Front, and southward until decreasing latitude brought about a balance between the rate of expansion of the glacier and the rate of its wastage. During all this time the polar-front zone across the northern United States and Canada would have been displaced to the south, describing a wide arc beyond, along, and across the southern-marginal part of the ice sheet.

At the maximum the marginal zone of most active flow of the ice sheet would have extended from Great Bear and Great Slave lakes southeastward through the Great Plains of Canada and thence east across the region of the Great Lakes. Activity of flow within the marginal zone would have been a result of greater nourishment here than

elsewhere. Accumulation in this zone would have thickened the ice here, thus inducing flow toward areas of thinner ice—that is, toward the ice-sheet margin and also toward the central region of the ice sheet. (This movement is discussed more fully by Demorest in a paper soon to be published.) Now it cannot be supposed that accumulation was uniform throughout the length of the marginal belt. The vagaries of snowfall from cyclonic storms emanating from various directions and following tracks that shifted from time to time, coupled with the vagaries of wind drifting of fallen snow, would have provided some sectors of the marginal belt with greater increments of ice than other sectors. The result would have been broad, very low domes on the upper surface of the ice sheet from beneath which the basal ice would have flowed slowly away (by extrusion) in all directions, and the positions of these domes should have shifted with time. The most obvious evidence of such domes would be radially oriented striae overlapping each other in some sectors. On such evidence the "ice centers" reviewed at the beginning of this paper have been identified. Not only are these centers of radial flow consistent with the hypothesis of highland origin and windward growth, they are demanded by it. Similar centers are expectable to the west and south, though not until the movements of erratics have been as thoroughly studied as in Scandinavia can we be sure of their presence or absence. However, attention should be drawn to the unpublished evidence of a former center in northern Alberta, as well as to the inferences of Burwash (1935), who from fragmentary evidence of striations, suggested early centers of outflow in the region of Great Slave Lake.

Beyond the southeastern sector of the ice sheet, where conspicuous highlands stood in the common paths of many storm tracks, the cold climates induced by the growing ice sheet coupled with exceptional snowfall resulted in the local development of glaciers. Newfoundland, the highlands of Gaspe, and perhaps the highlands of New Brunswick and the White and Adirondack Mountains were among these. All, however, probably coalesced with, or were overwhelmed and surrounded by, the encroaching Laurentide ice sheet at its maximum.

This reconstruction considers the Laurentide ice sheet as the outgrowth of glacier ice from highlands on Baffin Island, on the Coast of Labrador and in eastern Quebec, and possibly also from other highlands. This hypothesis of highland origin and windward growth is built up chiefly by comparison with Scandinavia and by deductive reasoning. With our present scanty knowledge of all but the southern part of the vast area formerly occupied by the Laurentide ice sheet, we cannot fully support it or any other hypothesis with field data. However, we can predict

that if the hypothesis be true, then rocks from the mountains of origin should occur as erratics well to the west and south of these highlands, and other evidence of outward flow of the ice in these regions should exist. Gould (1928, p. 37) reports evidence of southwesterly flow of ice in southwestern Baffin Island, but this very probably is referable to the renewed outward flow of highland ice at a very late time, when the Laurentide ice sheet had nearly or entirely disappeared.

CONDITIONS DURING DEGLACIATION

All the centers of radial flow inferred from striae must record late Wisconsin flow. The Keewatin center shifted southeastward, the Labradorian center shifted northwestward, and in the region southwest and south of Hudson Bay ice flowing southeast was replaced by ice flowing southwest and west.

These movements, though not necessarily synchronous, are probably referable to the same late part of Wisconsin time. Referable also to a great arc roughly paralleling the periphery of the ice sheet, they combine to give a consistent general picture of a reduced Laurentide ice sheet shrinking toward a central area in the Hudson Bay Region. Perhaps the shrinkage was more rapid west of the Hudson Bay region than in and east of this region, for then, as now, precipitation in the west may have been less than it is farther east.[6]

GLACIERS OF SOUTHERN SOUTH AMERICA

For comparison, the glacial conditions in southern South America ought to be mentioned. In Patagonia and southern Chile, during the last glacial age, the glaciers originated in the Andes, but on the plains to the east they never expanded beyond the piedmont condition. Ice covered the peninsula completely only at the extreme south, where the peninsula is narrowest and where the temperature is lowest. The small extent of ice there compared with its vast extent in North America and Europe probably is attributable to three factors:

(1) On the windward (Chilean) flank of the Andes, as on the windward flank of the Scandinavian mountains, the glaciers flowed rapidly down steep valley gradients and calved into deep water.

[6] Unpublished data compiled by D. A. Nichols of the Bureau of Geology and Topography, Canada, since this paper was written, and kindly made available by him to the writer, suggest these events: When the southern margin of the shrinking ice sheet had reached the James Bay region, the glacier separated into two lobes or distinct parts, one lying over the Hudson Bay region and the other lying over the highlands of eastern Quebec. As suggested by Nichols, the ice in the Hudson Bay region must have disappeared relatively soon, before much crustal upwarping had occurred, because the extensive postglacial marine deposits around the bay, which show that Hudson Strait must then have been open to the sea, now lie at altitudes up to several hundred feet. The highlands of eastern Quebec may have supported a glacier or glaciers for some time thereafter, and very likely the same was true of the mountains of Labrador and Baffin Island as well.

(2) Although the annual precipitation in the southern Andes is comparable with that in the Scandinavian Mountains, winter temperatures are relatively high. Even during the last glacial age this fact probably resulted in snowfall being largely confined to the higher altitudes, whereas in Norway much snow must have fallen even at sea level.

(3) The southern Andes are so high that they create a strong rain shadow in their lee, and their height and continuity north of the widely glaciated region prevented, during the last glacial age, the functioning of a flanking "helper process" such as nourished the entire southern margin of the ice sheet in Europe.

CONCLUSION

The concept presented in this paper is that during the Wisconsin Glacial Age the ice sheet that covered much of North America east of the Cordillera originated as valley glaciers in the highland areas and expanded through nourishment by warm moist air masses moving from the south and west. At its maximum it constituted the Laurentide ice sheet as named by G. M. Dawson and extended by T. C. Chamberlin.

The available facts do not prove that the ice sheet originated and developed in this way, but the data assembled are believed to show: (1) that this mode of origin and growth is both topographically and climatologically probable; (2) that the Scandinavian ice sheet originated in a mountainous highland and later shifted away from it; and (3) that the hypothesis offered explains more satisfactorily the facts we possess than does any other thus far proposed.

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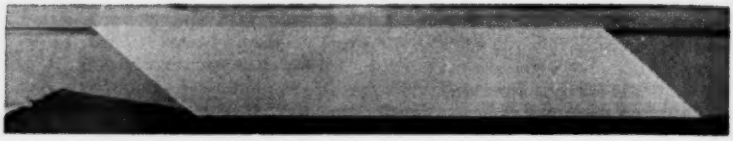

FIGURE 1. EQUALLY DISTRIBUTED FLOW
Amount of shear between successive planes (which are theoretically of infinitesimal thickness) is the same throughout.

FIGURE 2. PARABOLIC DISTRIBUTION OF SHEAR
Amount of shear between successive planes increases as a parabolic function of depth.

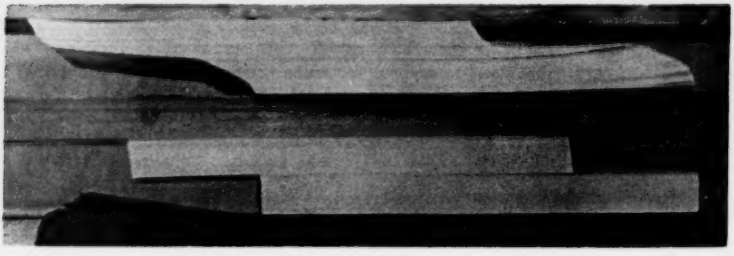

FIGURE 3. IRREGULAR FLOW AND DISCONTINUOUS SHEAR
Irregular distribution of flow at top is transitional to truly discontinuous shear at bottom.

STREAMLINE FLOW AND SHEARING
Illustrated by differential sliding of a pack of cards.

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ICE SHEETS

BY MAX DEMOREST *

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* Deceased November 30, 1942. Paper checked and prepared for publication by Richard Foster Flint.

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ABSTRACT

Stresses responsible for ice flow within glaciers are analyzed and classified as follows: (1) extrusion flow, (2) obstructed extrusion flow, (3) obstructed gravity flow, and (4) gravity flow. Though these types form a gradational series, each is characteristic of different conditions of glacier configuration and underlying topography.

With these concepts of flow in mind it is argued that:

(1) Rates of flow within an ice sheet are at a maximum under and downstream from the névé line, which is generally near the periphery. Upstream from the névé line rates of flow diminish to zero under the areas of dispersal, which generally coincide with the areas of maximum accumulation on the ice-sheet surface.

(2) Areas of accumulation on extensive ice sheets are generally closer to the ice sheet margins than to the centers, and movement and erosion take place only under the marginal and near-marginal parts of an ice sheet. Erosion is considered to be limited by the depth of weathered and loosely jointed mantle rock available for plucking and removal by ice. When surfaces of tightly jointed rock are reached plucking is inhibited (therefore no further tools are available for abrasion), and the ice cover prevents weathering. Thus erosion is practically stopped, and the ice sheet, as compared to subaerial erosive agents, becomes relatively protective of the land surface.

(3) The configuration of the Greenland ice sheet is most likely due to subsurface topographic control.

(4) Ice sheets generally originate by growth and expansion of piedmont glaciers formed at the base of glaciated highland areas. Probably the Laurentide ice sheet thus came into being by expansion of glaciers originating in the highlands of Labrador and Baffin Island. Growth toward the south and west was favored by moisture-bearing cyclonic winds blowing from those directions.

(5) Differences between shrinking ice sheets in the Antarctic and Greenland result from differences in the cause of shrinkage. In the Antarctic shrinkage results from reduced precipitation, particularly at high altitudes. Consequently low-altitude piedmonts are relatively favored, while the high-altitude ice sheet of the plateau approaches stagnation. In Greenland increased ablation, particularly at low altitudes, causes shrinkage. Consequently low-altitude piedmonts have generally disappeared, while the high-altitude ice sheet remains relatively well nourished.

INTRODUCTION AND ACKNOWLEDGMENTS

This paper presents some hypotheses on the origin, growth, movement, and disappearance of ice sheets. No claim to originality is made for these hypotheses. They are, however, presented from a new point of view based on the author's recent analyses of glacier regimens and the

mechanics of ice movement within glaciers (Demorest, 1942). It is hoped that this point of view will aid in a general understanding of the phenomena discussed and will bring new light to other glaciological problems.

Professor R. F. Flint and Mr. A. L. Washburn, both of Yale University, are thanked for reading the manuscript and making valuable criticisms; the National Research Council is thanked for a Fellowship under which the study was carried out.

ICE MOVEMENT WITHIN GLACIERS

GENERAL PROBLEMS OF FLOW

The author believes that laminar or streamline flow of ice within glaciers has been demonstrated and is now accepted by most glaciologists. However, the mechanics of such flow is not fully understood, for two major problems remain: (1) how flow-producing stresses originate and affect various types of glaciers, and (2) how these stresses cause plastic flow of solid crystalline ice. The author has already (1942) discussed the first problem, and it is here reviewed. The second problem, which has received considerable attention in the past, is now under experimental investigation by the author.*

Laminar or streamline flow always results from stresses that cause distributed differential shearing (Pl. 1). The simplest case (Pl. 1, fig. 1) is the familiar differential sliding of a pack of cards. However, with true flow, whether plastic or not, the cardlike layers are so thin as to be theoretically nonexistent as discrete layers. Moreover, within the flowing medium, shearing is not controlled by any planes of preexisting weakness such as are found between cards. Rather, flow can take place in any and all directions as determined by the effective shear stresses, and it may occur on curved or plane surfaces. Figure 1 of Plate 1 illustrates equally distributed shearing motion in which the amount of differential shear between any two adjacent infinitesimal layers is the same throughout the flowing substance. Obviously the effective shear stresses are also equally distributed. A case theoretically more common in nature (Pl. 1, fig. 2) is what may be called a parabolic distribution of shear, for the shear stresses increase as a parabolic function of depth. Other sorts of shear distribution are also possible, and there may be a gradation from a very irregular distribution of shear to a truly nondistributed shear in which there are discrete planes along which a high degree of differential motion occurs (Pl. 1, fig. 3). There is unquestionable evidence of such irregularly distributed and nondistributed

* EDITOR'S NOTE: It is hoped that arrangements may be made to digest and publish the author's data on this experimental work, which was carried on under grants from the Society.

shear within glaciers, and it must be admitted that nondistributed shear is not a matter of flowage. This fact has been appealed to as a proof that true flowage does not occur within glaciers. More logically, however, it can be regarded only as a proof that frequent departures from ideal stress distributions occur within glaciers and that such stresses are occasionally of sufficient magnitude to cause ice to yield nonplastically.

A plastic substance such as ice differs from a true fluid in that high stresses may exceed its plastic limits causing nondistributed failure. Obviously this occurs most frequently at and near the surface of a glacier where the ice, under small confining pressure, is least plastic. At depth within a glacier where the ice is highly plastic, probably no shear stresses in nature are sufficiently great to exceed the plastic limits. Therefore at depth there is probably no nondistributed shearing, though irregularities in the glacier floor may cause local variations in the distribution of shear. However, such variations need not concern us in an analysis of the shear stresses responsible for ice movement, for fundamental principles can be better understood if discussion is confined to ideal conditions.

For such ideal conditions, the shear stresses responsible for flow may be distributed in four different ways depending upon the slope of the glacier floor and the profile of the glacier's upper surface: (1) extrusion flow, (2) obstructed extrusion flow, (3) obstructed gravity flow, and (4) gravity flow. Each of the four types of stress distribution causes a distinct "type of flow." (However, all types are fundamentally of the same nature—*i. e.*, all are examples of streamline flow.) These types are gradational from one to another, yet each type is characteristic of distinct glacier conditions of surface profile and underlying topography.

EXTRUSION FLOW

Extrusion flow occurs within glaciers only where there is very little surface slope and where the glacier floor is either approximately horizontal or sloping in the upstream direction. In general this applies to the accumulation areas of ice sheets, piedmonts, some névé fields, and some valley glaciers. The forces responsible for extrusion flow are a result of differential pressures, and these in turn are a result of the surface slope (Fig. 1). The line *ac* is the trace of a vertical cross section through the glacier, and the line *abc* is the trace of a surface of deformation. Differential pressure results from the surface slope because every particle of ice along the vertical line *ac* has a greater weight of overlying ice on its upstream side than on its downstream side. Thus there is an unbalanced force directed downstream, and this force is the same on

all ice particles from bottom to top of the glacier. The rate of ice movement, however, is not the same from bottom to top, for ice plasticity decreases toward the surface, and decreased plasticity involves increased resistance to deformation. Hence, though differential pressure is equal from bottom to top, ice at any given level is extruded from below the

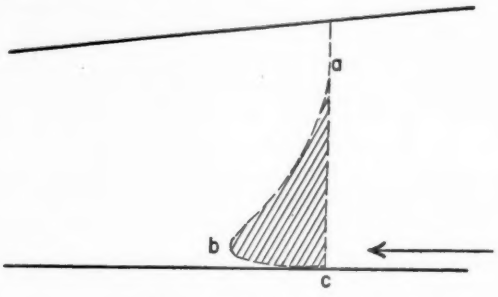

FIGURE 1.—*Extrusion flow*

Long section of glacier. Line *abc* represents surface of deformation. Differential pressure, due to slope of upper surface, is responsible for flow. Basal ice is retarded by friction, but near-basal ice above limit of retardation is moved farthest in unit time—it is extruded from below the overlying ice.

overlying more resistant ice. For this reason such flow is called extrusion flow.

OBSTRUCTED EXTRUSION FLOW

Wherever extrusion flow is in any way obstructed, simple movement of the sort just described cannot occur. Instead, differential pressure causes forward and upward movements of ice such that the obstruction is overridden. The most obvious cause of obstruction is a physical barrier of some sort—for example, a topographic barrier against which a glacier with sufficient pressure head would flow uphill, overtopping the barrier. A more frequent but less obvious cause of obstruction results from a poor adjustment of the surface profile of the glacier. Examples of this are frequently found in the ablation areas of glaciers where ice thins rapidly in the downstream direction. In such places the ice at any one point, being thinner and therefore less plastic than the ice immediately upstream, moves more slowly, thus constituting a partial obstruction to the upstream ice. The result is a forward and surfaceward movement by which the "obstruction" is overridden. Such thinning may, and often does, exist throughout large parts of a glacier; consequently the obstructing effect is important, causing surfaceward flow throughout all parts of the thinning ice (Fig. 2).

As shown in the author's earlier paper, continuous unobstructed extrusion flow occurs most typically in the accumulation areas of glaciers, and the velocity of such flow must necessarily increase in the downstream direction so as to be approximately proportional to the distance from the locus of maximum accumulation. In other words, downstream

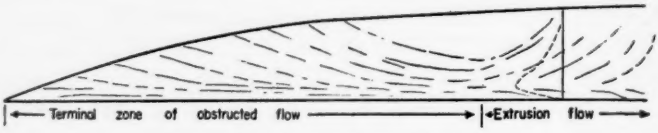

FIGURE 2.—*Obstructed extrusion flow*

Long section through ablation area and terminus of pressure-controlled ice sheet. Lines of flow show surfaceward movement of obstructed ice and transition from unobstructed to obstructed flow.

from the locus of maximum accumulation there must be a continuous increase in velocity. Increasing velocity, however, requires an increasing surface slope, for the velocity of flow depends on the amount of differential pressure which in turn depends on the surface slope. Thus continuous unobstructed extrusion flow requires an adjustment of slope to the length of the glacier and to the distribution of accumulation. Variation in thickness of ice resulting from major irregularities in the glacier floor also affects the profile adjustment. If the glacier profile departs from its required adjustment to these factors, flow immediately becomes obstructed (as when ice thins rapidly in the downstream direction), and obstructed extrusion flow then occurs until the profile is readjusted.

Within the ablation areas of ice sheets or piedmonts, loss of ice by melting and evaporation causes a continuous departure from an adjusted profile condition. Hence, obstructed extrusion flow is characteristic of such areas (Fig. 2).

Since both extrusion flow and obstructed extrusion flow are a result of differential pressures, they are grouped together as "pressure-controlled" types of flow. Fundamentally these flow types are mechanisms of leveling in which both the surface slope and differential pressures are reduced toward zero. To understand this imagine a great mass of ice occupying a topographic basin with no outlet, or imagine it as occupying an enormous tub. Assume that the ice is thick enough to be plastic at depth and that it has a sloping upper surface. This slope may extend radially outward from a high center as is common on ice sheets, or it may be constant from one side of the tub to the other, or it may be of any other form. It does not matter what the form is, for so long as there is a slope there must be differential pressures, and, as a result, there

must be movement. If no ice is added by snowfall and if no ablation takes place, pressure-controlled movements must cause a transfer of ice from the higher areas to the lower areas until ultimately the surface is leveled; that is, until the surface slope and accompanying differential pressures are reduced to zero. Thus a great tub of ice, like a tub of water, must ultimately acquire a level surface.

A great continental ice sheet on an essentially horizontal floor is similar to the tub of water, but with the tub removed. Water, in such a case, would spread in all directions, attempting to flatten out to a zero surface slope; so would the ice, but its spreading would be extremely slow. However, if sufficient time is allowed and it is again assumed that the ice sheet is affected by neither snowfall nor ablation, it would spread until the whole mass were finally attenuated and thinned so that movement would cease. The amount of spreading required for such cessation of movement is determined by the rate of ice deformation, which is proportional to both the plasticity and the differential pressure; that is, to the weight of overlying ice and to the slope of the surface. Thus, as the ice spreads, becoming increasingly thinner, the rate of movement is reduced because the weight of overlying ice is reduced. The surface slope in its approach to zero is also reduced, so that there are two factors causing a decreased rate of movement. As a result ice in all parts of the ice sheet moves at a constantly decreasing rate until ultimately it becomes completely motionless.

GRAVITY FLOW

In contrast to pressure-controlled movements gravity flow, in its ideal form, occurs only where the surface slope of a glacier is less than, or equal to, the slope of the glacier floor. Such flow results from shear components of direct gravitative force in a way closely analogous with the parabolic shear of a pack of cards. This is understood by reference to Figure 3 where the line *ab*, parallel to both the glacier surface and the glacier floor, represents one of the infinitesimal cardlike layers. The vertical arrow terminating at *a* is a vector representing the total gravitative force at any point on the plane *ab*. Obviously the total force must equal the weight of overlying ice. At every point within such a body under gravitative stress there is an infinite number of potential shear planes lying in all possible directions; the strength of the shear stress along any potential plane depends on (1) the strength of the fundamental stress (in this case the total gravitative weight) and (2) the sine of the slope of the given plane. Thus the steeper a potential plane is, the stronger will be the shear stress along it, for the sine of its slope increases with increasing steepness. Of the infinite number of potential shear planes

at the point *a*, consider those in the direction *ax*, *ay*, and *az*. Obviously the stress along *ax* is the strongest since its sine is the largest, and the stress along *az* is the weakest since its sine is the least.

It is apparent that stress along *ax* cannot cause motion within the ice because the glacier floor is in the way; for the same reason motion cannot

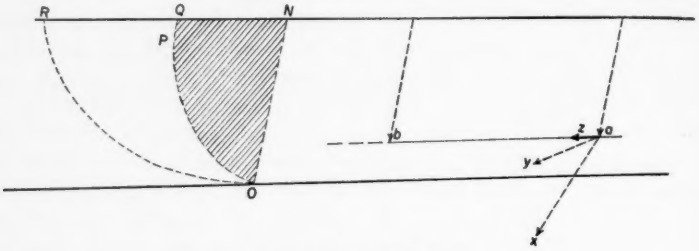

FIGURE 3.—*Gravity flow*

Long section of glacier. Shear component *az* is strongest component capable of causing motion (on plane represented by *ab*). Differential motion on infinite number of planes parallel to *ab* results in surface of deformation OPQ. Maximum differential motion at bottom and minimum at top; however top moves farthest, for it is carried downstream by sum of all underlying differential motions. Line OR represents surface of deformation such as would occur in substance of uniform viscosity.

occur along *ay*, or along any other potential plane that slopes more steeply than the glacier floor. Thus the stress along plane *az*, parallel to the glacier floor, is the strongest shear stress capable of causing motion, and it does cause motion. All the ice above the plane *ab* slides downstream—that is, it is sheared over the underlying ice by the *az* shear stress. However, shearing is not confined to the plane represented by *ab*, for similar shearing occurs on each of the infinite number of cardlike planes parallel to *ab*. Higher in the glacier, though, there is less overlying weight of ice, and consequently the shear stresses are weaker, and lower in the glacier, where the overlying weight is greater, the shear stresses are stronger. It follows that the most stress must occur at the base of the glacier where the overlying weight is greatest, and the least must occur at the surface. Hence, the greatest amount of differential movement must also occur at the base and the least at the surface. Yet the surface is translated the greatest distance downstream, for it is carried by the sum of all underlying differential movements.

Were a glacier of uniform plasticity throughout its depth, such a distribution of shear stresses would be parabolic. Any vertical section, represented by *NO*, would, in a given unit of time, be drawn out into a parabolic curve such as *OR*. However, ice is not of uniform plasticity

throughout a glacier; its plasticity increases with confining pressure—with overlying weight. Stated more conveniently, plasticity decreases from bottom to top, but decreasing plasticity means increasing rigidity, or increasing resistance to deformation, and since, in this case, increasing resistance is accompanied by decreasing shear stresses, the rate of

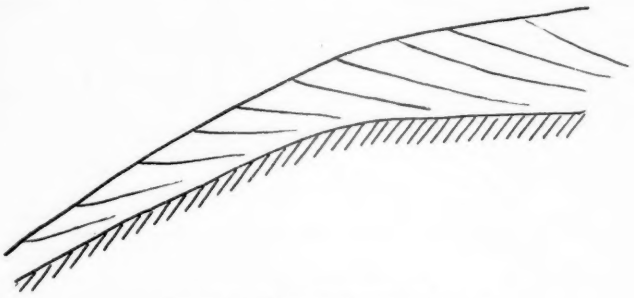

FIGURE 4.—*Obstructed gravity flow*

Transitional from obstructed extrusion flow. Transition occurs where lines of flow change from surfaceward and upward to surfaceward and downward.

deformation falls off rapidly toward the surface producing a surface of deformation such as *OPQ* rather than *OR*.

OBSTRUCTED GRAVITY FLOW

Since ideal gravity flow occurs only where the surface slope is less than or equal to the slope of the glacier floor, such flow cannot occur where ice thins in the downstream direction. Under this condition the ice at any point, being thinner and less plastic than ice immediately upstream, constitutes an obstruction to flow from upstream; the effect of such an obstruction is similar to that within pressure-controlled glaciers, for a forward and surfaceward movement of ice occurs. There is a difference, however, in that the surfaceward movements of obstructed gravity flow are forward and downward rather than forward and upward (*see* lines of flow in Figure 4), because the slope of surfaceward lines of obstructed flow varies inversely with the slope of the glacier surface, so that, within limits, the more steeply a surface slopes the more nearly horizontal the lines of surfaceward flow must be. However, when the surface slope passes a certain limiting value (which varies with the extent of obstruction) the lines of flow pass the horizontal and come to have a downward component of motion. The motion is thus aided by direct gravitative shear stresses of the sort associated with ideal gravity flow.

The change from forward and upward to forward and downward surface movement marks the transition from obstructed extrusion flow to obstructed gravity flow. The further transition from obstructed gravity flow to ideal gravity flow occurs where the surface profile approaches parallelism with the glacier floor, for in such places the lines of flow

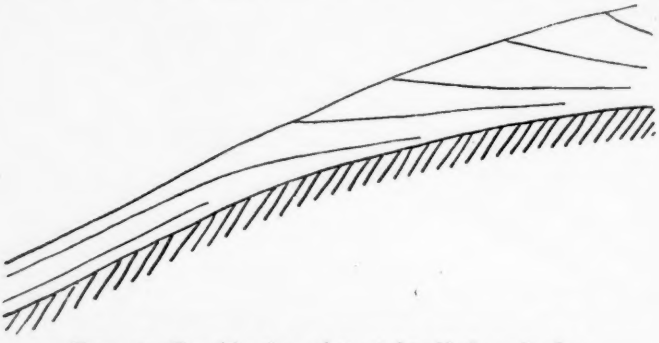

FIGURE 5.—*Transition from obstructed to ideal gravity flow*
Surfaceward and downward lines of flow become parallel to glacier surface.

also approach parallelism with the floor (Fig. 5), so that there is no longer a surfaceward component of motion.

Obstructed gravity flow is generally limited to situations in which the upper surfaces of glaciers can acquire a considerable slope, for, if the surface approaches the horizontal, any appreciable obstructing effect would cause obstructed extrusion flow rather than obstructed gravity flow. Obviously sufficient slope cannot be acquired where the glacier floor is essentially horizontal, except locally at the terminus of a glacier where excessive ablation sometimes produces a clifflike front. Generally, however, an extensive surface of sufficient slope for obstructed gravity flow cannot occur unless the glacier floor slopes relatively steeply in the downstream direction. Hence the existence of obstructed gravity flow is usually dependent upon the slope of the glacier floor.

SUMMARY

Unlike the types of extrusion flow, the types of gravity flow are not mechanisms that lead directly to the production of level surfaces. Rather they are mechanisms of downhill drainage and are, therefore, called "drainage-controlled" types of flow. Pressure-controlled flow is roughly analogous to the flow of water in a lake or reservoir where movement results in the attainment, or near attainment, of a level surface; or, in the case of a great continental ice sheet lying on a more or less level surface

without basinlike walls, the flow is analogous to the spreading of a fluid on a horizontal plane. Drainage-controlled flow, on the other hand, is analogous to the flow of water in a river or channel where, without replenished supply, all the water would drain away.

In tabular form the four types of ice flow and their controls are as follows:

Pressure-controlled (dependent on slope of glacier surface)	EXTRUSION FLOW	Shear stresses due to differential pressure and differential plasticity
	OBSTRUCTED EXTRUSION FLOW	
Drainage-controlled (dependent on slope of glacier floor)	OBSTRUCTED GRAVITY FLOW	Shear stresses due to components of gravity
	GRAVITY FLOW	

DISTRIBUTION OF FLOW WITHIN ICE SHEETS

RATES OF FLOW

Rates of flow within any glacier are fundamentally dependent upon the strength of the flow-producing stresses. Thus pressure-controlled glaciers, which generally have surfaces of very little slope, are characterized by slow rates of ice motion. The known slopes on the Greenland ice sheet have been summarized by Hess (1933, p. 110). He shows that, within the marginal zone, slopes up to an altitude of 500 meters above the border average between 1° 30′ and 4° 30′. Within the vertical range between 500 and 1000 meters above the border, slopes average between 1° 10′ and 2° 30′; and within the 1000- to 1500-meter range they average between 0° 30′ and 3° 30′. All the higher figures given are taken from parts of an outlet glacier where drainage-controlled rather than pressure-controlled flow occurs. Consequently only the lower figures are truly typical of the pressure-controlled ice sheet. In King George V Land in the Antarctic where the inland ice descends under pressure control of the sea, the surface slope at the border, below 500 feet of altitude, is everywhere less than 1° 30′ (Mawson, 1915, map in pocket).

Unfortunately actual rates of extrusion flow cannot be measured because such flow occurs only at depth. Probably, however, even the maximum rates found near the borders of an ice sheet are not greater than a few inches or fractions of an inch per day (Demorest, 1942, p. 54); and farther inland the rates of flow diminish until under the locus of maximum accumulation they are theoretically zero—at least their horizontal components of motion are zero, though there must be slow, vertically downward components of motion. In Figure 6 it is seen that there is a "downdraft" under the locus of maximum accumulation. Thus the ice

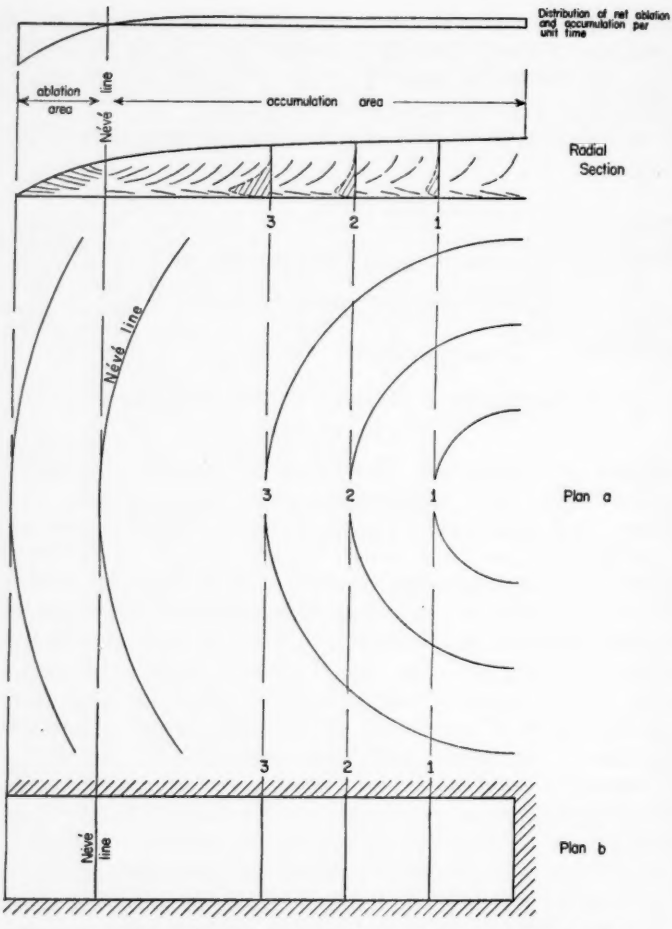

FIGURE 6.—*Plan and radial section of ideal pressure-controlled ice sheet*

Radial section shows lines of flow. Greater bulges in downstream surfaces of deformation show increased discharge with distance from locus of maximum accumulation. (In this hypothetical case the center of the ice sheet.) Plan b, hypothetical rectangular pressure-controlled glacier with frictionless walls. Relationships between successive vertical cross sections and surface areas upstream from them are same as for circular ice sheet, plan a.

lost by radial extrusion from that depth within the ice sheet is made up by a slow subsidence from above and by accumulation on the surface. Similar but not such strong downdraft movement occurs in all other parts

of the accumulation zone and accounts for the way in which surface accumulation feeds the extrusion outflow at depth.

Equilibrium of the accumulation zone occurs when the total volume of ice extruded in unit time is just equalled by the total volume accumulated on the surface. This concept of equilibrium aids us in understanding why the rate of extrusion must increase downstream from the locus of maximum accumulation, for if equilibrium is to be maintained the volume of ice extruded in unit time through any cross section must equal the volume accumulated on the surface upstream from that cross section. Thus the farther one goes downstream the greater is the upstream accumulation, and hence the greater the volume of ice extruded in unit time.

Within the ablation area of an ice sheet extrusion flow gives way to obstructed extrusion flow, and the downdraft effect of the accumulation area gives way to the surfaceward movement of ice. Equilibrium within such an area is established when ablation on each part of the surface is exactly offset by surfaceward movement. In nature ablation is generally greatest at the glacier terminus, and hence, if equilibrium is to be maintained, the greatest amount of surfaceward movement must also occur at the terminus. Greater velocity, however, requires a steeper surface slope, and as a consequence the profile becomes more and more strongly convex toward the terminus.

From Figures 2 and 6 it is seen that the lines of more rapid flow toward the terminus approximately parallel the glacier floor. The rate of such flow must equal the rate of extrusion flow near the base of the glacier under the outer edge of the accumulation area—that is, under the névé line where the rate of flow is at a maximum.

SPREADING OF ICE SHEETS

Though the circumstances just discussed are demanded by equilibrium conditions, the converse is not necessarily true. In a hypothetical ice sheet that is affected by neither ablation nor accumulation there could be no equilibrium; yet the distribution of extrusion flow would be much the same as if there were equilibrium, for the velocity of flow would necessarily increase radially from the center of dispersal. (There would be no locus of maximum accumulation.) The ablation zone being absent, obstructed extrusion flow would not occur except within a narrow border of thin ice near the terminus. Obviously extrusion of ice from the interior would not be compensated for by surface accumulation, and terminal movements would not be offset by ablation. As a consequence the ice sheet would necessarily spread, extending itself in all radial direc-

tions, and at the same time become thinner; as previously pointed out, such spreading would ultimately lead to stagnation.

The spreading of actual ice sheets in the past was probably of this general character though there could have been no cessation of ablation and accumulation. Rather, spreading was a result of only relative changes in these factors. For example, increased accumulation with unchanged ablation would lead to a thicker and broader ice sheet; decreased ablation without change of accumulation would lead to a broader ice sheet of unchanged thickness; and decreased ablation with decreased accumulation might lead to a thinner and broader ice sheet.

Relatively sudden changes of accumulation (as, for example, within a few years) may so alter the surface profile as to cause local and temporary departures from the adjusted profile required for continuous extrusion flow. The result would be a change to obstructed flow which, within the accumulation area, would automatically bring about a reestablishment of the adjusted profile. Thus, in general the accumulation areas of spreading and shrinking ice sheets must be characterized by continuous unobstructed extrusion flow, and hence the maximum velocities must generally occur within the marginal parts beneath and downstream from the névé line.

EROSION BY ICE SHEETS

Maximum velocities at and near the margin of an ice sheet imply that the maximum erosional strength is also localized near the margins (Coleman, 1926, p. 17; Hobbs, 1935, p. 5). Hobbs, however, denies the existence of any internal ice movement inland from the marginal zone: ". . . internal movement goes on along the steep front of continental glaciers and as far back as their appreciable slopes extend." The present author would amend this statement to say that internal movement must go on wherever there is any slope. Erosion depends on ice movement, and the rate of erosion depends (in part) on the rate of movement, so that the erosional strength of an ice sheet, like the rates of flow and surface slopes, must increase from zero under the locus of greatest accumulation to a maximum under the névé line. Between these places it would be difficult to mark the point where the surface slope, rate of flow, and erosional strength become "appreciable." Yet, the author agrees that erosion under most ice sheets must have been confined to the near-marginal areas. This idea is based on a belief that the locus of maximum accumulation on large ice sheets is generally nearer the margins than the centers. Whether the nourishing precipitation on ice sheets is derived from anticyclonic circulation, by which it would be spread radially toward the margins, or whether it is derived from cyclonic winds blowing across the margins is

an unsettled question; in either case the maximum accumulation would lie relatively close to the borders of a broad ice sheet.

On a hypothetical circular ice sheet having a locus of maximum accumulation near the border, equilibrium would be established with a theoretically horizontal surface within the ring that forms the locus (Fig. 7, profile 1), and outflow by extrusion would be limited to the mar-

FIGURE 7.—*Growth of ideal circular ice sheet receiving maximum accumulation near periphery*

Increased accumulation with maximum near periphery causes change from equilibrium condition (profile 1) to profile 2. Higher ice near periphery causes centripetal flow and a rise of the central surface so that continued accumulation and growth results in re-establishment of equilibrium (profile 3).

ginal zone beyond the locus. Should accumulation increase to the extent of producing a thicker ice sheet, it is obvious that the first part to become thicker would be the ring-shaped area of maximum accumulation. In consequence of this the surface would slope not only toward the margins away from the locus of maximum accumulation but also toward the center producing a saucerlike basin within the ring of accumulation (Fig. 7, profile 2). Such a slope would necessarily induce motion toward the center, but since this motion would involve convergence of ice from all directions there would obviously be an obstructing effect. Thus the flow would be obstructed extrusion flow, the surfaceward movements of which would result in a rise of the surface within the basin. This would continue until the basin floor was built up to a level even with the surface in the area of maximum accumulation—that is, until the interior again became horizontal and equilibrium was re-established (Fig. 7, profile 3).

Obviously there is no likelihood that such a circular ice sheet with a symmetrical distribution of accumulation has ever existed, yet it is likely that the areas of maximum accumulation have been located relatively near the margins of large ice sheets and that little erosion has occurred under the broad interior regions.

In saying that the rate of erosion is proportional to the rate of ice movement over the floor of the glacier, one must make allowance for two facts: (1) Loaded ice, like a loaded stream, cannot erode, and (2) erosion cannot occur unless there is erodable material for the ice to pick up. Because of (1) the zone of maximum erosion under an expanding ice sheet does not ordinarily extend out to the margins; the marginal ice may be so loaded as to even cause deposition (ground moraine). It is only where the weathered and loosely jointed bedrock of the regolith is scanty or has

been largely removed that a glacier can be sufficiently underloaded to allow erosion.

The fact that erosion requires erodable material is obvious, but it has not been generally recognized that glaciers, unlike streams, may run out of erodable material. A glacier erodes only by plucking and scouring, while at the same time it protects the glacier floor from weathering. Once a glacier has picked up the loose material of the regolith and has ground the glacier floor down to a smooth surface of rounded, tightly jointed bedrock, there is no longer any material to be plucked, and without tools derived by plucking there can be no scouring. The glacier, covering the land and prohibiting weathering, then becomes a protective rather than a destructive influence.

Probably the end point of plucking under actual glaciers is seldom reached, especially in regions of high and rugged topography where lee slopes, which favor plucking, are long enduring. Yet it seems likely that the Greenland ice sheet, with its rugged underlying topography, now has such a small amount of erodable material left beneath it that it accomplishes less erosion than would result from subaerial processes. In other words, it is relatively protective (Demorest, 1937, p. 46). According to Gould (1940, p. 866) the same is now true of the Antarctic ice sheet, and it was very likely true of the former great Pleistocene ice sheets during recession across the relatively drift-free regions of Canada and Fenno-Scandia.

GREENLAND'S ICE SHEET

CONFIGURATION OF THE ICE SHEET

Distribution of accumulation.—The distribution of accumulation on the western side of central Greenland in the year 1930-1931 was determined by members of Alfred Wegener's Deutsche Grönland-Expedition (Wegener, 1933, p. 164). Maximum accumulation occurs at an altitude of about 2500 meters and about 120 miles from the border of the ice sheet. These values in general agree with earlier determination of Koch and Wegener (1922, p. 377) and of De Quervain (1925, p. 167) at other localities on the western side of the ice sheet, so we may be sure that they represent general conditions. The determinations of Koch and Wegener are perhaps the more interesting (Fig. 8), for they show not only that accumulation increases both west and east from the high crest of the ice sheet, but also that it is much greater on the western side of the sheet than on the eastern side. Those who believe that ice sheets are nourished chiefly by cyclonic winds should find nothing unexpected in this distribution of accumulation, for the majority of cyclonic disturbances in Greenland come from the west and southwest. On the other hand those who hold that nourishment is derived solely from anticyclonic circula-

tion should be surprised if not disturbed by this distribution. (*See* Demorest, 1941.)

Of greater interest is the fact (Figs. 8, 9) that the zones of maximum accumulation do not coincide with the crest of the Greenland ice sheet. It was stated earlier that one should expect the areas of maximum accumulation to be the areas of highest altitude. The fact that this is not

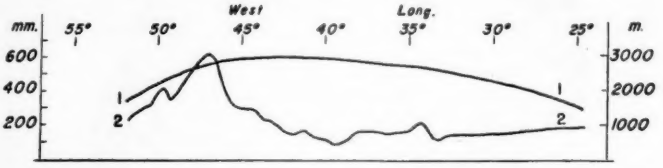

FIGURE 8.—*Cross section of northern dome of Greenland ice sheet showing annual accumulation*

Curve 1 shows topographic section along line from 72°30′ N. Lat. on west side of ice sheet to 76°40′ N. Lat. on east side. Curve 2 shows accumulation of snow along same line during year 1912-1913. Note that maximum of accumulation is to west of ice divide, and that a submaximum occurs on east slope. (After Koch and Wegener.)

so in Greenland must mean either: (1) that, in the relatively recent past, distribution of accumulation was not the same as today, or (2) that the configuration of the ice sheet is largely determined by the configuration of the underlying topography. If (1) is correct we must assume that the present configuration of the ice sheet is inherited from a time when the areas of maximum accumulation coincided with the present crest of the ice sheet. This interpretation may be favored by those who believe that cyclonic disturbances migrate considerable distances across ice sheets and thereby furnish the nourishing precipitation, for it can be argued that in past times of greater glaciation the general atmospheric circulation was more vigorous than now. As a consequence cyclonic disturbances were stronger and able to migrate greater distances on to the ice sheet. The more numerous and perhaps stronger disturbances from the west and southwest thus created a maximum of accumulation farther east than at present, and at the same time the lesser disturbances from the east migrated farther west. As a result maxima of accumulation from both east and west may have combined to build the present domes of the ice sheet. If this were the case we must then assume that the present distribution of accumulation has not existed long enough to cause any appreciable modification of the earlier formed domes.[1]

[1] Since the Greenland ice sheet is small and very narrow as compared to the great Pleistocene ice sheets, the suggestion of maximum accumulation near the center of Greenland is not contrary to the author's belief, expressed above, that maximum accumulation should generally occur relatively near the margins of a large ice sheet.

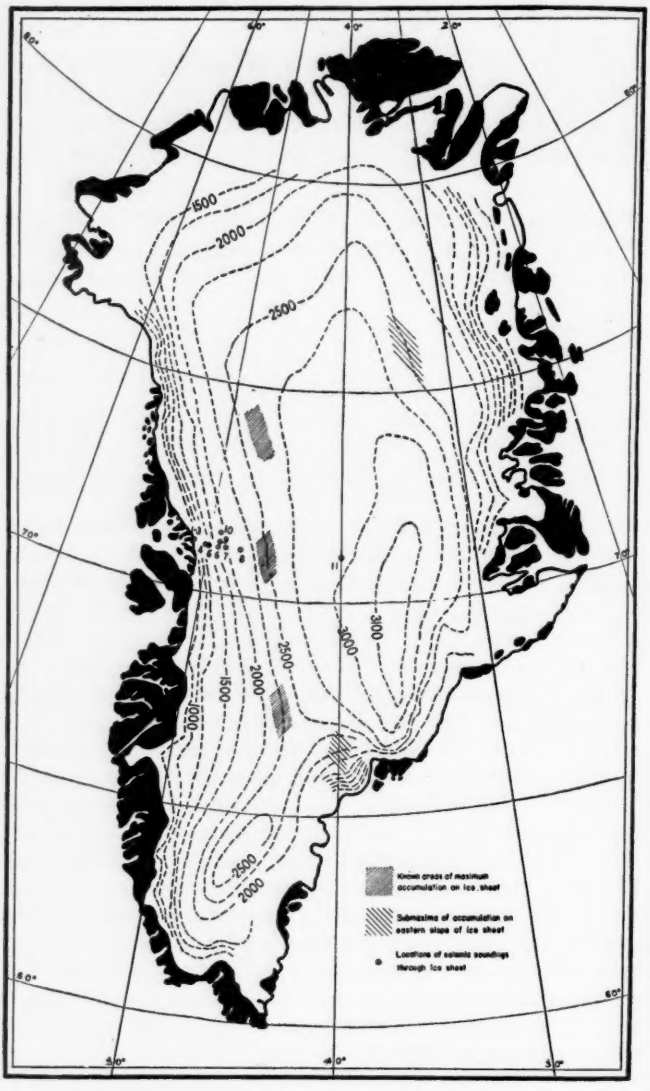

FIGURE 9.—*Topography of Greenland ice sheet*

Shows positions of seismic soundings through ice sheet and known areas of maximum and submaximum accumulation. (Topography from Danish Geodetic Institute, 1937.)

Those who adhere to the belief that ice-sheet nourishment is derived solely from anticyclonic circulation will reject this explanation of the Greenland domes as being based on impossible assumptions, for they hold that cyclonic disturbances do not migrate more than a few miles across the margins of an ice sheet and that precipitation from anticyclonic circulation does not create a maximum of accumulation on the central part of even a relatively small ice sheet.

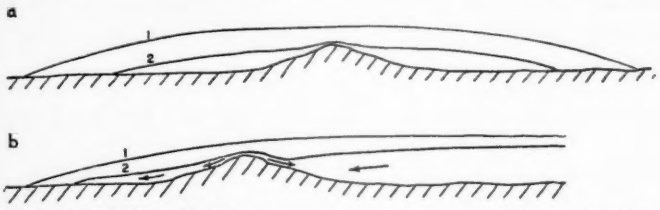

FIGURE 10.—*Effect of highland areas on topography of growing or shrinking ice sheets*

Highland areas under center (a) or periphery (b) may become sites of radial outflow during growth or shrinkage of ice sheets. It is only where ice is very thick and extensive compared to size of highland area that the topography of an ice sheet will be relatively unaffected by underlying highlands.

It may also be argued that the building of two domes on the ice sheet is not an expectable result of precipitation from cyclonic disturbances, for, even if such disturbances do migrate through considerable distances onto the Greenland ice sheet, there is no apparent reason why accumulation should be favored in two separate areas. Over different parts of an ice sheet of truly continental dimensions it is likely that variations in meteorologic conditions could cause the development of separate accumulation maxima and, therefore, separate domes; however, on a relatively small ice sheet such as covers Greenland it seems more reasonable to suppose that separate domes would not be favored; rather, a single elongate dome should be expected.

Effect of underlying topography.—Though the present author favors the belief that cyclonic disturbances can and do migrate considerable distances on to ice sheets, he is impressed by the argument that meteorologic conditions in Greenland would in themselves create only a single dome. Therefore he prefers the hypothesis that the configuration of the Greenland ice is largely influenced by underlying topography.

The general manner in which topography may affect the configuration of an ice sheet is illustrated by Figure 10a (profile 2) in which a broad highland area is glaciated. There is sufficient snowfall on the summits to bury them completely, yet the area of maximum accumulation occurs

over the mountain flanks at a distance from the summits. Obviously the position of the thickest ice is determined by the area of maximum accumulation, but the thinner ice over the buried peaks is nevertheless higher. This effect of the peaks on the configuration of the ice sheet can be eliminated only if snowfall in the zone of maximum accumulation should increase to such an extent as to build the surface over the mountain flanks to as high an altitude as that over the buried summits (Fig. 10a, profile 1).

During deglaciation this sequence of profile changes would be followed in reverse, for as reduced accumulation or increased ablation causes the ice sheet to become thinner, although ice under the area of maximum accumulation remains thickest, its surface will be lowered more than the surface over the mountain peaks. During later stages of thinning the summits might emerge as nunataks in the center of the shrinking ice sheet.

This hypothetical picture showing a high mountain mass at the center of an ice sheet is perhaps the simplest and most easily understood example of the way in which subsurface topography may affect the configuration of an ice sheet; yet the effect may be just as important in places where large topographic features underlie parts of an ice sheet at considerable distances from the center. Figure 10b thus represents a highland that is overwhelmed during a time of maximum glaciation by ice from a distant source (profile 1). We may imagine that the highland area is limited laterally so that the more plastic ice at depth within the ice sheet parts and flows around it. Of course, some such ice striking directly against the upstream side of the highland will be forced up the flanks and over the summit of the area, but the greatest volume of flow will no doubt be deflected to pass around at a lower altitude. With deglaciation and thinning it is obvious that profile 2 (Fig. 10b) will come into being. Deflected flow about the base of the highland might continue during this stage, but there would no longer be flow over the summit area because the surface gradient on the upstream side of the summit area has been reversed. Thus, though the general direction of ice movement at lower altitude is maintained as before, there is a local high-altitude reversal of flow and the establishment of radial outflow from the summit areas.

Obviously further modifications of form would result as deglaciation proceeds. Where meteorological factors are favorable the highland ice cap might continue to receive local nourishment while the broad ice sheet would shrink and its borders would recede. The highland might then be left as an isolated area of longer-enduring glaciation, and radial outflow would gradually be extended to lower altitudes. On the other hand,

if meteorological conditions did not favor maintenance of a local ice cap, the already thinner ice over the summits of the highland would waste away long before the thicker ice on the lowlands had retreated. In this case the summits would soon emerge as nunataks, and the period of high-altitude radial outflow would probably be short lived.

Conditions in Greenland by which possible underlying topography may have affected the ice sheet cannot be conclusively demonstrated. It may be, as Koch (1928, p. 434-446) supposed, that the two domes of the inland ice represent two highland areas that were separate centers of glaciation. He believed ice sheets expanding from those centers have simply coalesced to produce the present configuration. If the highlands are as real as the present author supposes, this was probably the case during growth of the ice sheet. It must be recognized, however, that the ice sheet, at maximum glaciation, may have been thick enough and extensive enough to obviate all topographic effects, in which case the present-day configuration is due to differential thinning over the highlands rather than to growth from them.

The concept of radial outflow and the manner in which highlands affect an ice sheet during deglaciation is interesting not only as a possible explanation of the configuration of the Greenland ice sheet, but also as a suggestion of how such highland areas as the Adirondacks, the White Mountains, and the Shickshock Mountains of Gaspe Peninsula may have been deglaciated in Pleistocene time. (*See* Flint *et al.*, 1942.)

Seismic soundings in Greenland.—Against the hypothesis, preferred by the present author, of subsurface control of the configuration of the Greenland ice sheet, the assertion may be advanced that seismic soundings in Greenland prove the subsurface to be saucerlike, the interior being depressed by the weight of overlying ice. (For example, *see* Daly, 1934, p. 9-11.) Obviously were this the case there would be no high subsurface topography to control the configuration of the ice sheet. In challenging this argument the present author does not deny that the weight of overlying ice may have caused depression of the land, but he does point out that existing seismic data do not demonstrate that the depression was sufficient to produce a saucerlike subsurface topography.

So far as can be learned from published sources 14 seismic soundings have been made through the Greenland ice sheet (Brockamp *et al.*, 1933) by members of Wegener's expedition of 1929 and 1930-1931, which established a headquarters station on the western border of the ice sheet in approximate latitude 71° north. A substation called Eismitte was located near the center of Greenland in the same latitude. Of the 14 soundings, 10 are presented as giving good enough results so that

altitudes on the subsurface topography could be determined with a fair degree of accuracy. Positions of these 10 soundings, plus an eleventh less reliable sounding at Eismitte, are shown in Figure 9, and data from them are summarized in Table 1. It is seen that three stations were within 5 kilometers of the ice border. Six lay along an east-west line between

TABLE 1.—*Seismic soundings through the Greenland ice sheet*

Station	Altitude (in meters)		Ice thickness (meters)	Subsurface slopes	Location (Distance from border)
	Ice surface	Subsurface			
1	1030	640	390		5 km.
2	880	375	505		5 km.
3	954	800	46		5 km.
4	1250	450	800		*Ca.* 25 km.
5	1590	290–370*	1220–1400		*Ca.* 35 km.
6	1830	800	1030	18°–22°	62 km.
7	1950	420	1530		82 km.
8	2100	250–300*	1770–1850	7°–12° (possibly 22°–30°)	120 km.
9	1840	480	1360		*Ca.* 60 km. (*ca.* 17 km. N. of sta. 6)
10	1878	830	1048		*Ca.* 60 km. (*ca.* 39 km. N. of sta. 6)
11	3000	1100(?)	1900(?)		390 km. (Eismitte)

* Known range of altitude rather than possible limit of error.

the headquarters station and Eismitte, their respective distances from the ice border being approximately 25 km., 62 km., 120 km., and 390 km.[2] The last station is Eismitte. Two other stations were located on a line running north of the 62-kilometer station; one lay about 17 kilometers north, and the other about 39 kilometers north.[3]

[2] The exact locations of several of the stations cannot be determined from published sources, for in some cases the various maps and articles issued by members of the expedition do not agree. This is especially true of the five stations located nearest the ice border.

[3] Confusion resulting from variations in the published maps and articles has apparently led Daly (1934, p. 10-11) to believe that other stations were located at 62, 82, and 120 kilometers north of the 62-kilometer station on this same north-south line.

Obviously, as shown on the map and in Figure 11, the stations are so distributed, and they are so few in number, that data from them cannot be used as a basis for any reliable estimate of the general shape of the surface underlying the Greenland ice sheet. Yet, in the few places where the soundings do give some idea of underlying topographic details, there is a strong suggestion of high relief. This is certainly shown by the three

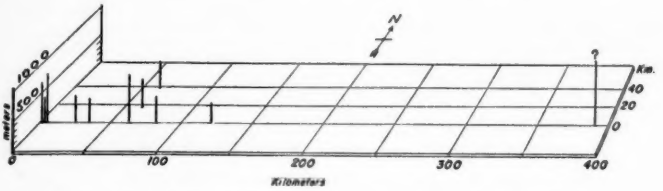

FIGURE 11.—*Known subsurface altitudes under Greenland ice sheet*

Vertical lines indicate altitude above sea-level at sites of seismic soundings through ice. Zero kilometer line extending east-west across diagram is approximately on parallel of 71°10′ N. Lat.

relatively closely spaced stations near the coast where the subsurface appears to be as rugged as the exposed mountainous land beyond the ice-sheet margin. It is also suggested by information from the stations at 62 and 120 kilometers where the seismic data were sufficient for the determination of subsurface slopes. At 62 kilometers slopes of 18° to 22° are indicated, and at 120 kilometers slopes of 7° to 12° (Brockamp *et al.*, 1933, p. 97, 99, 101). The 7° to 12° slopes at 120 kilometers are given as minimum values, for, though these values are presented in Brockamp's text and in one of his illustrations, his map (p. 99) shows slopes of 22°, and Sorge (1933, p. 344) mentions them as being as high as 30°. These slopes, even if they are no more than 7°, very definitely suggest a rugged topography. This is further suggested by the fact that the several stations show a relief of at least 410 meters (*ca.* 1370 feet) in the area covered. The stations are so widely spread that there is no reason to believe that this is the maximum relief.

From the 120-kilometer station to Eismitte, 270 kilometers away, no soundings were made, and therefore, on the basis of seismic work, not even a guess can be made as to the nature of the subsurface; much less can any indication be found of a saucerlike depression. Unfortunately the sounding at Eismitte adds no valuable data, for as pointed out by Sorge (Brockamp *et al.*, 1933, p. 159) that sounding is unreliable. The subsurface altitude of 1100 meters given in Table 1 and in Figure 11 is simply the best estimate derived from uncertain seismic data.

In summarizing the seismic information on the land surface underlying the Greenland ice sheet, it may be said that: (1) The only reliable data are derived from 10 seismic stations established on the west side of the sheet within 120 kilometers (*ca.* 73 miles) of the ice border in approximate latitude 71° north. (2) The minimum value for total relief in the region is 410 meters (*ca.* 1370 feet). (3) Maximum slopes of 12° to 22°—possibly 30°—are found at distances of 62 and 120 kilometers from the border. (4) There is no evidence that the surface is at all saucerlike, but there is a suggestion that it is rugged. (5) An eleventh station, at Eismitte, shows a possible subsurface altitude of 1100 meters (*ca.* 3700 feet), but this is admittedly unreliable, and even if it were reliable it would not confirm the idea of a saucerlike depressed interior.

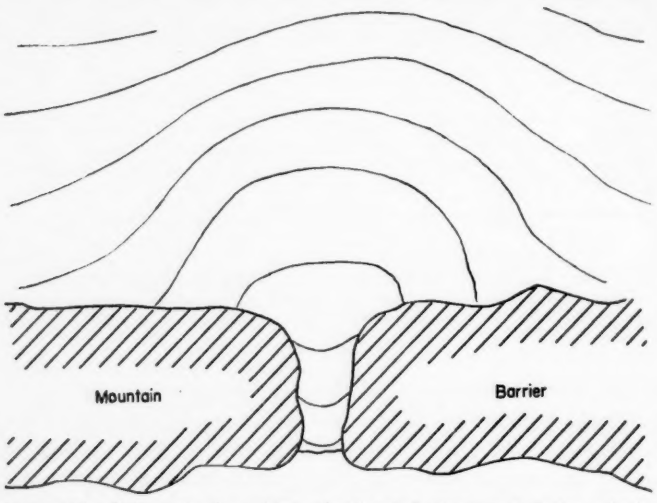

FIGURE 12.—*Schematic plan of Greenland outlet glacier and drainage basin*

Pressure-controlled ice sheet is dammed back by barrier of coastal mountains. Escape through outlet valley in barrier allows drainage flow and results in thinner ice through basin.

ICE SHEETS AND PIEDMONT GLACIERS

GROWTH OF ICE SHEETS

General considerations.—In parts of Greenland and in the Ross Sea sector of the Antarctic, the great pressure-controlled inland ice sheets terminate against barriers of high coastal mountains where the only escape of ice is through relatively narrow outlet valleys that cut across

the mountains to the sea (Figs. 12, 13). In general the outlet glaciers in these valleys are drainage-controlled because the valley floors are inclined steeply enough to promote drainage flow. Such glaciers are essentially the same as steeply inclined valley glaciers that drain from high-level névé fields, for in both cases the supply of ice is derived from a high-level pressure-controlled source; and at the head of the rela-

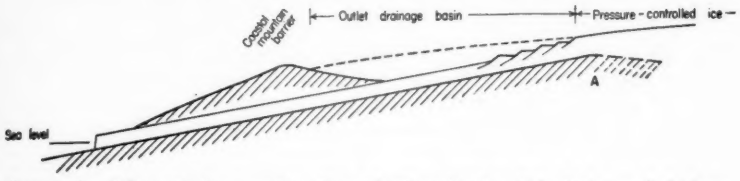

FIGURE 13.—*Schematic long section through Greenland outlet glacier and drainage basin*

Relatively rapid drainage flow through outlet basin undersaps pressure-controlled ice causing development of "landslide" terraces.

tively steeply inclined valley there is a transition from pressure-controlled to drainage-controlled flow (Fig. 4).

Where such drainage glaciers pass out of their steep valleys onto gentle or flat slopes, the opposite transition occurs, for drainage flow must give way to pressure flow (Fig. 14). As is well known this occurs most conspicuously where drainage glaciers advance onto broad lowlands at

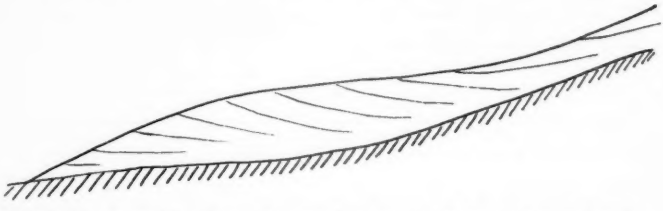

FIGURE 14.—*Transition from obstructed gravity to obstructed extrusion flow*

Transition occurs where lines of flow change from surfaceward and downward to surfaceward and upward.

the lower ends of their valleys. The result is the formation of pressure-controlled expanded-foot glaciers and, after farther advance, the formation of piedmont ice sheets such as are now found on the coast of Alaska. The only difference between these pressure-controlled ice sheets and those already discussed is that their greatest supplies of nourishment may be derived from inflowing drainage glaciers rather than from direct snowfall.

The relationships among a high-level ice sheet (or névé field), its

outlet glaciers, and possible low-lying piedmont glaciers are similar to those between a high-level reservoir from which water drains through a narrow inclined channel to feed a second reservoir at a lower level (Fig. 15).

Equilibrium, expansion, and shrinkage of piedmont glaciers involve the same considerations of supply, ablation, and movement that are ap-

FIGURE 15.—*Reservoirs connected by drainage channel*

Rising water in lower reservoir drowns lower part of channel in same way that rising ice in piedmont glacier drowns inflowing drainage channels.

plied to other pressure-controlled glaciers. However, piedmonts may expand at the expense of the drainage glaciers that feed them, for, as a piedmont expands and thickens during increasing glacierization, the greatest increase in thickness must occur at the source, or sources, of supply, so that the ice sheet becomes extended upstream through the drainage valleys (Fig. 16, profiles 1-3). As this process continues a piedmont may

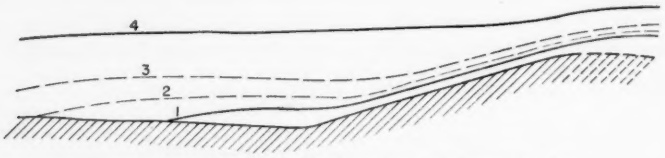

FIGURE 16.—*Growth of ice sheet from piedmont glacier*

Profile 1, piedmont glacier fed by drainage flow through outlet valley from high-level ice sheet or névé fields. Expansion of piedmont through profiles 2 and 3 drowns outlet valley and produces piedmont coalescent with high-level ice sheet, profile 4.

expand and thicken until its upper surface rises above the heads of the drainage glaciers, thus completely eliminating drainage flow. In Greenland and the Antarctic this would result in a single large ice mass in which the high-level sheet and the piedmont sheet combine to overwhelm the mountain barrier through which the outlets flow (Fig. 16, profile 4). Returning to the analogy with a reservoir of water, we have the lower and upper reservoirs filled to a common level so that their surfaces become continuous, and the drainage channel between them is drowned. On an ice sheet, however, the surfaces of the lower and upper "reservoirs" would not acquire a uniform level, nor even a slope of continuous curvature, for the underlying topography would be responsible for a lesser

thickness of ice over the buried mountains than over the piedmont plain, and this must be expressed by differences in the curvature of surface slopes over the two areas (Demorest, 1942, p. 42).

Piedmont growth from glaciers of mountains or high plateaus may thus be a forerunner of ice-sheet formation in which the mountains or plateaus would eventually become buried. The thickness and extent of the ice sheet must depend entirely upon the sources and distribution of supply and ablation. This concept of the origin of ice sheets is not new, for it dates from 1910 when Hobbs (1910a, p. 269) first discussed it in connection with his classification of glaciers. However, Hobbs' descriptive study of glaciers and the present author's theoretical study of the mechanisms of ice flow lead to the same general hypothesis of ice-sheet formation.

If the author is correct in his belief that Greenland ice is underlain by a land of high topography, this hypothesis may be accepted as explaining the Greenland ice sheet, and it may also explain the Pleistocene ice sheets of Fenno-Scandia and Labrador where mountainous highlands occupied large areas under the ice. It may be less apparent that the same hypothesis can be used to explain the Keewatin and Patrician ice sheets that developed on lowlands of little relief.

Keewatin ice sheet.—In discussing the origin of the Keewatin ice sheet, Coleman (1926, p. 14) says, "a vast *neve* accumulated to the west of Hudson Bay on comparatively low ground . . . with no mountains, and therefore no Alpine glaciers, as a starting point." Theoretically there is no reason why this hypothesis of Coleman's may not be correct, for continued accumulation of névé on a low-lying area should certainly lead to an ice sheet. To the present author, however, it seems unlikely that meteorological conditions over a lowland would have favored such névé accumulation. As Stefansson (1940, p. 225 *et seq.*) points out, the lowlands of high polar latitudes are definitely warm in summer. This is true even where the average annual temperatures are low enough to maintain subfreezing soil temperatures within a few inches of the surface. Yet, should winter snow accumulation of the lowlands be great enough the short season of summer warmth might be insufficient for removal of snow, so that a permanent and growing névé would result. Once started such a névé would be self refrigerating and would thereby favor its own growth by reduction of summer temperatures. Against this, however, it may be argued that winter snow accumulation on the Keewatin lowlands is seldom heavy, for in general the extreme cold of that region is a result of anticyclonic conditions that bring relatively little precipitation. Consequently there is little probability of extensive névé formation. In ad-

dition, it should be expected that the rain shadow of the Cordillera should have lain across the Great Plains and Keewatin lowlands of the past just as it does today.

These arguments are obviously inconclusive, for past conditions may have included unknown factors that would invalidate present speculative concepts. Yet, on the basis of existing knowledge, Coleman's hypothesis seems an unlikely explanation of the Keewatin ice sheet. It seems more likely that the Keewatin sheet was simply a westward extension of an earlier-formed ice sheet (the Laurentide ice sheet) that originated in the highlands of Labrador and Baffin Island (Flint, 1943).[4] Certainly it is expectable that precipitation on the high, mountainous regions of Labrador and Baffin Island must have been far greater than on the lowlands west of Hudson Bay; precipitation must have been even more excessive, as at present, on the western slopes of the Cordillera. As a result the first-formed ice sheets during a time of increasing glaciation should have existed on the eastern and western extremities of the continent.

The western (Cordilleran) ice sheet presumably filled and submerged the valleys and mountains of the Pacific slope and built a piedmont apron extending into the sea. Toward the east, however, its extension was very likely limited by its own "snow shadow." Extension of the Laurentide ice sheet would also have been favored toward the west, and toward the southwest, for those are the probable directions from which migrating cyclonic disturbances approached. As the ice sheet expanded over the central Canadian lowlands, the advancing high borders would have intercepted greater amounts of precipitation than had previously fallen on the lowland surface. The fact that this should have occurred is demonstrated by present conditions in the Black Hills where high mountains intercept relatively large amounts of moisture, whereas similar amounts pass practically unprecipitated over the near-by semiarid lowlands to the north and south. Thus the ice sheet's growth was presumably favored by its own high borders, and, as a result, it could probably expand westward over lands on which névé accumulation was far less favored. At the same time self-refrigeration should have brought lower summer temperatures and a longer season of snowfall to the margins of the ice sheet.

This ice sheet is envisioned as ultimately expanding across the Keewatin lowlands and advancing against the eastern slopes of the Cordillera. The shape of this extended ice sheet must have been largely determined by the distribution of accumulation along its borders, and its

[4] Unknown to the present author at the time this paper was being written, Professor Flint was preparing a paper on the origin of Pleistocene ice sheets. His beliefs regarding the origin of the Keewatin ice sheet are identical with those of the present writer.

altitude was determined by the altitude of maximum precipitation. Over the broad interior of the sheet there must have been a nearly flat expanse of ice at the level of maximum accumulation. If we can judge from existing altitudes of maximum precipitation, this level must have been of the order of 10,000 feet—certainly not more than 15,000 feet. Those who would argue that Pleistocene ice sheets were much thicker than this (*e. g.*, Shepard, 1936, p. 501; 1941, p. 154) must demonstrate that the maximum precipitation could have occurred at higher altitudes—a demonstration that the physical laws of meteorology make improbable.

The known shape and extent of the Keewatin ice sheet is compatible with the expectable distribution of accumulation along its borders, for as it advanced into the rain shadow of the western mountains there was less and less moisture available to its western extremities. Consequently it extended less far toward the south on the western part of the continent than on the eastern part.

In conceiving of the Keewatin ice sheet as an extension of a Labrador-Baffin Island ice sheet, there is no necessity to think of the Labrador-Baffin Island area as the single "center" of ice dispersal for the whole great ice mass. Indeed, within a great irregularly shaped ice sheet there is no center of dispersal. Rather, directions of ice movement, being controlled by directions of surface slope, are usually normal to the borders, and the borders usually are not curved about a single center. Thus the Labrador "center" was only a generalized center of curvature for the borders in the eastern part of the continent. Similarly, the Keewatin "center" was the generalized center of curvature for the western part, and the Patrician "center" was presumably the center of a large but relatively local bulge in the borders over the upper Mississippi Valley. Obviously centers of an even more local character must have existed for each of the numerous border lobes that were so characteristic of the ice sheet.

Disappearance of the great Laurentide ice sheet seems to have proceeded from west to east, as has been pointed out by Coleman (1926, p. 18) and others. The meteorological conditions already outlined support this, for the relatively poorly nourished western part of the ice sheet should have been dissipated long before the well-nourished eastern part. In short, favorable topographic and meteorologic conditions caused the ice sheet to originate in the east, and these same favorable conditions caused it to endure longer there.

It will be apparent that this hypothesis of the origin of the Keewatin ice sheet depends upon a belief that ice-sheet nourishment is largely a result of precipitation from cyclonic winds; those who hold that nourish-

ment is solely a result of precipitation from anticyclonic circulation will reject it. Unfortunately this question of nourishment remains unsettled.

SHRINKAGE OF ICE SHEETS

Antarctic conditions.—In the Ross Sea sector of the Antarctic, the ice sheet was once very much thicker than at present. David and Priestly (1914, p. 290) suggest that ice in the outlet valleys of the McMurdo Sound region was 2000 to 3000 feet thicker, and Gould (1940, p. 863) points out that all but the higher peaks of the Queen Maud Range "must have been deeply overwhelmed with ice at the culmination of glaciation." Ice of the Ross Shelf was also thicker, for on the flanks of Mt. Terror it left moraines 800 feet above its present level (Hobbs, 1910b, p. 120), and according to David and Priestley the southern part of its surface was once more than 1000 feet higher than now. If this thicker ice could have been afloat it must have extended 3200 to 4000 feet deeper into the sea. However, known soundings of the Ross Sea do not show depths of more than 3000 feet (American Geographical Society, 1928), and we may therefore conclude, as does Gould (1940, p. 864), that the shelf was aground throughout its extent. Even now it is aground on Roosevelt Island (Gould, 1940, p. 852) and at several other places (English, 1941, p. 473, 475). Thus at the time of maximum glaciation the Ross Sea must have been filled with a true land-borne piedmont that was fed by outlet glaciers of the high-level ice sheet; or, if the ice was thick enough, the coastal mountain barriers may have been overwhelmed and the outlet glaciers drowned beneath a continuous ice sheet that included the piedmont.

The present Ross Shelf could have come into existence only after the piedmont ice had become thin enough to float. Leveling of its upper surface may be a result both of spreading and of isostatic adjustment to the sea water.

The deglaciation that caused ice thinning in the Antarctic is unusual in that it probably results from a condition of extreme cold. Gould (1940, p. 867) and others have noted that the continental glacier is starving through a lack of nourishment, the absolute humidity over the ice being extremely small because of the prevailing low temperatures. Ablation has little to do with deglaciation, for even summer temperatures at the borders of the continent seldom rise above the freezing point. Thus the ice sheet approaches a condition in which it is affected by neither ablation nor accumulation, and as a result it is spreading radially into the sea and becoming continuously thinner. Paradoxically, as Gould points out, amelioration of the Antarctic climate would cause increased

precipitation and accumulation, and therefore expansion of the glaciers. This would continue until ablation increases were sufficient to offset the increased accumulation.

Should conditions of extreme cold and excessive ice thinning be long-continued in the Antarctic, the high-level ice sheet behind the mountain barrier might become so thin as to expose the divides at the heads of the outlet valleys. As a result the supply of ice to the valleys would be cut off, and the Ross Shelf and other areas of piedmont ice would become isolated. At the present time the shelf, lying close to sea level where snowfall is relatively great, may receive as much supply by direct precipitation as by inflow from drainage valleys. Hence, even though the piedmont ice should become isolated, it might be longer-lived than the high-level ice sheet that appears to be starving more rapidly through lack of precipitation. Probably only wastage by wave action and calving along its seaward margin can cause the shelf to disappear before the high-level ice sheet.

West Greenland "piedmonts" and drainage basins.—The ice sheet in Greenland, like that in the Antarctic, was once much thicker. Except for parts of northern Greenland (Lauge Koch, 1928, p. 376 *et seq.*) and possibly the high peaks of the Mt. Forel region on the eastern coast (Wager, 1932, p. 288), all the exposed coastal mountains were once covered by ice. Thus the present mountain barriers of the western coast, particularly the part of the western coast that lies within the few hundred miles south of Melville Bay) must have been overwhelmed, and the high-level ice sheet must have been continuous with a great piedmont ice sheet lying seaward from the barrier (Fig. 16, profile 4). This "piedmont," however, must have been quite different from that of the Ross Sea, for the "piedmont lowland" at the foot of the mountain barrier is an area of great relief partially drowned in Baffin Bay. It is a typical embayed coast with mountainous islands and peninsulas rising 1500 to 5000 feet out of the sea. Also, dissipation of this extended ice mass appears to have been very different from that of the Ross Sea, for the Greenland piedmont ice has completely disappeared during a time when the high-level ice sheet has not yet uncovered the heads of the outlet drainage valleys. In short, the piedmont ice has disappeared very rapidly, while the high-level ice sheet has retreated very slowly. As a consequence, downstream parts of the drainage valleys were the first to be uncovered and the first to acquire re-established drainage flow after thinning of the piedmont ice. Since that time disappearance of the piedmont ice has been accompanied by progressive headward extension of drainage flow in the outlet valleys, because the high-level, pressure-controlled ice sheet, lying over the heads

of the outlet valleys, has been eaten into by the relatively rapid downstream drainage that undersaps and steepens the front of the pressure-controlled ice. Such a steepened front exhibits a transition to drainage flow which, in turn, undersaps pressure-controlled ice farther upstream. Thus the re-establishment of drainage flow progresses upstream until all the outlet valleys are affected and the face of the pressure-controlled ice has retreated back to the buried divides (Fig. 13) at the heads of the basins. Yet even at the divides the undersapped front of the pressure-controlled ice must still be steep enough to move by drainage flow. As a result, such flow must continue to extend itself even farther headward until the surface of the pressure-controlled ice is lowered by drainage thinning and an adjusted profile of pressure-controlled flow is established up to the divide.

Such lowering of the ice surface beyond the divides is analogous to the lowering that occurs in the water surface of a reservoir just at the head of a spillway. This analogy was used by Hobbs (1911, p. 132) and has been followed by the present author (Demorest, 1937, p. 47), who considered the lowered ice surface throughout an entire drainage basin (called by Hobbs basin of exudation) to be the result of the reservoir-spillway effect. This, however, cannot be true because a surface thus lowered must be underlain by ice of pressure-controlled flow rather than of drainage-controlled flow, and that is not the case within the Greenland outlet drainage basins. Ice in the drainage basins is analogous to water in a spillway, for the flow is drainage flow. The depressed surface of Hobbs' analogy occurs only at the head of the spillway in the pressure-controlled ponded water above the dam. Therefore, in the outlet glaciers it applies only to the limited surface lowering that occurs within the pressure-controlled ice immediately upstream from the buried divide which constitutes the "dam." Within the Greenland glaciers, however, drainage flow has probably not yet extended itself headward as far as the buried divides, or at least not far enough to have completely lowered the pressure-controlled ice surface beyond the divides. This is suggested by the topography of the ice near the heads of the basins where a series of steplike terraces rises toward the higher, gently sloping, pressure-controlled ice-sheet surface. Such terraces have been previously described by Nansen and Peary and are discussed by Hobbs (1911, p. 129-132). The treads of the terrace steps are tilted back toward the next higher risers so that small lakes form on them in summer months when ablation is at a maximum. The author believes that they are true "landslip" terraces resulting from oversteepening of the pressure-controlled ice front (Fig. 13). Land slipping is gravitative

shearing, so it is argued that drainage control is being extended upstream. Obviously the actual landslips, which take place on discrete planes of shear, are not examples of true flow, but they are examples of the drainage process and are no doubt accompanied by and succeeded by true flow movements such as characterize ice motion in downstream parts of the outlet basins.

The fact that piedmont ice on the Greenland coast has disappeared so rapidly as to leave pressure-controlled ice still within upstream parts of the drainage basins is partially explained by the fact that thinning of the piedmont ice was accompanied by increased ablation. This is because the high coastal islands and peninsulas that form the "piedmont lowland" emerged early as nunataks, each one of which induced ablation by reradiation of the sun's heat from bare-rock surfaces; more significantly, the sun-heated bare rock caused convection currents of warm air to circulate over wide expanses of ice thus promoting increased ablation by both melting and evaporation (Ahlmann, 1935, p. 89). As ice thinning proceeded, the nunataks emerged ever higher above the ice surface, and their areas of outcrop increased, so that ablation increased at an accelerating rate. In time ice remained only in the valleys and lowlands. Where the valleys are above sea level the ice finally thinned and disappeared, and where the valleys are submerged floating ice tongues may have come into existence to be finally wasted away by wave action and calving.

Ice behind the coastal mountain barrier has melted more slowly because its terminus, except in the outlets, lies at higher altitudes. Consequently the interior surface is also higher, so that possible nunataks remain buried, and ablation, which is initially small at high altitudes, is not greatly augmented. A further reason for longer maintenance of ice behind the barrier is the fact that abundant supply is derived by extrusion flow and obstructed extrusion flow from farther inland. The effect is as if the high-level ice sheet were flowing outward against a gigantic dam (the mountain barrier) in which there are numerous leaks (the outlet valleys). As has been mentioned before, pressure-controlled flow is a mechanism of leveling in which the ice strives to rise higher at its margins (against the dam) while it is lowered over the interior. Piedmont ice is simply the overflow and leakage that may disappear relatively rapidly when overflow has stopped. It is only under such extreme conditions as those in the Antarctic that piedmont ice sheets are apt to be longer-lived than the high-level ice sheets, for there the piedmonts are not greatly ablated, and they are better supplied by snowfall than are the high-level ice sheets.

SUMMARY

Principles that explain the various types of stress distribution responsible for ice flow within glaciers are best summarized in the author's first treatment of the subject (Demorest, 1942, p. 64):

"Analysis of glacier regimens shows that four types of ice movement may occur within glaciers, each type being characteristic of different conditions of underlying topography and surface configuration. These types are extrusion flow, obstructed extrusion flow, obstructed gravity flow, and gravity flow.

"The two types of extrusion flow are called pressure-controlled types because they result from and are directly controlled by differential pressures. True extrusion flow is characterized by radial or downstream extrusion of ice at depth from below overlying ice. It is found only within glaciers of very slight surface gradients, e. g., the interior parts of ice sheets and ice caps, expanded foot and piedmont glaciers, and some mountain névés and glaciers. Rates of flow increase in the downstream direction, being generally proportional to the distance from the locus of maximum accumulation; however, in all parts of a glacier, even where movement is most rapid, the absolute rates of flow are necessarily very low. In most cases even the maximum rates are probably to be measured in inches or fractions of an inch per day.

"Obstructed extrusion flow is found where pressure-controlled glaciers encounter any sort of obstruction. Probably the most common, though not the most obvious, obstruction results from excessive thinning of ice in the downstream direction. This is characteristic of the ablation areas of pressure-controlled glaciers and results in forward and upward movements in such a manner that internal ice is extruded to the upper surface of the glacier.

"The two types of gravity flow are called drainage-controlled types because they occur only on relatively steep topographic slopes where ice 'drains' away from higher-level sources of supply. Many mountain glaciers and outlet glaciers from ice sheets are of this sort. The mechanism of true gravity flow involves differential shearing of infinitesimal 'layers,' the greatest differential between layers occurring at the base of the glacier, but the greatest absolute motion occurring at the surface which is moved downstream by the sum of all underlying differential movements. The stresses responsible for movement are shear components of direct gravitative forces."

These concepts of ice flowage make for a sound understanding of several aspects of ice-sheet behavior. It becomes apparent that the most rapid movement, and therefore the most effective erosion beneath ice sheets, is confined to the marginal areas under and downstream from the névé line. It is pointed out that erosion can take place to only limited depths —to the depths of weathered and loosely jointed bedrock—for the ice protects the bedrock surface from weathering while it removes all preglacially weathered material. After such removal, the tightly jointed unweathered bedrock is not readily plucked, so that no tools are available for further abrasion. Hence the ice becomes relatively protective of the bedrock surface.

In discussing the configuration of the Greenland ice sheet it is contended that the existing high domes must be due to either: (1) past meteorologic conditions wherein the maxima of accumulation on the ice were so distributed as to be directly responsible for the domes, or (2) the influence of subsurface topography. Arguments are developed to

show that the latter is more probable, and as a general principle it is pointed out that all highland areas overlain by ice sheets are likely to be sites of topographic domes and radial outflow of ice during stages of ice sheet growth and shrinkage. Only if ice is very thick and extensive in comparison to the size of a covered highland does the highland have little or no effect on the topography of an ice sheet.

All known seismic soundings through the Greenland ice sheet are analyzed, and these data (contrary to general belief) do not reveal any saucerlike depression of the land.

It is argued that ice sheets are most apt to result from growth of glaciers in mountains or highland areas. Piedmont ice sheets accumulating at the base of a highland may become so thick as to overwhelm the highland, at the same time spreading laterally to cover extensive lowlands. Growth in this manner is most favored to windward where the high front of the expanding ice sheet intercepts precipitation from advancing cyclonic disturbances. It is believed that in this way the North American ice sheets were initiated in the highlands of Labrador and Baffin Island and that they expanded southward and westward from there to cover northern United States, the present area of Hudson Bay, and the broad Keewatin lowland. "Centers" of glaciation, such as the Keewatin center, are not necessarily centers of growth over which ice sheets originated. Rather they may be centers of dispersal outward from which flowage has taken place, and as such they may reflect only meteorologic and topographic conditions established on the broad surface of an extensive ice sheet that originated by piedmont growth from a distant highland.

In discussion of the shrinkage of piedmont glaciers conditions in the Ross Sea sector of the Antarctic and in Greenland are compared. It is pointed out that the Ross Shelf—a floating piedmont—was probably aground and therefore a true land-borne piedmont in its earlier history. The ice of the piedmont (as well as of the ice sheet on the high Antarctic Plateau) has recently thinned as a result of starvation in a climate of extreme cold. This thinning has made the piedmont ice a floating mass. Probably isostatic adjustment of the ice to the underlying water has been largely responsible for leveling of the ice-sheet surface.

Starvation of the shelf is in part due to reduced precipitation but may be more largely a result of reduced discharge, through outlet valleys, from the more acutely starved high-level ice sheet. Barring a climatic change that would bring increased precipitation to the Antarctic, it may be that the higher ice sheet will thin to stagnation before the relatively well nourished floating piedmont has disappeared.

In Greenland the high-level ice sheet, dammed behind a barrier of coastal mountains, has remained well nourished while past accumulations of piedmont ice have disappeared from the fiords and embayments of the coast. This is in part because high relief of the coastal "piedmont" provided large nunataks that hastened ablation of the thinning ice when the high ice sheet of the interior underwent relatively little increased ablation.

Thus ice shrinkage in Greenland results from increased ablation. This has affected the piedmonts more than the high-level ice sheet, and as a result the piedmonts have disappeared while the ice sheet remains, relatively well nourished. In contrast, ice shrinkage in the Ross Sea sector of the Antarctic has resulted from reduced precipitation that has had more effect on the high-level ice sheet than on the low-level piedmont shelf. As a consequence the shelf has remained relatively well nourished, while the higher ice sheet approaches stagnation.

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METEORITES AND AN EARTH-MODEL

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ABSTRACT

The author discusses the old hypothesis that the earth represents meteoritic material. With Jeffreys he assumes the planet to have been initially gaseous, with a mean temperature well above the boiling point of iron, namely 3000° Centigrade. A large part—perhaps much more than half—of the original body of gas may have been lost to space because of volatility at the high temperature. Condensation of the residual gas to liquid and differentiation in both the gaseous and liquid states occurred, but by processes significantly different from those pictured by Jeffreys. The suggested course of development gives an existing earth which has the following succession from surface to center: a true crust, the lithosphere; a thicker, vitreous asthenospheric shell; a still thicker, crystallized mesospheric shell; and a nickel-iron core, probably fluid and possibly behaving much like a gas.

The paper summarizes the principal observed facts about the nature of meteoritic stones and irons, and the reasons for supposing them to be fragments of a disrupted parental planet. A cause for this disruption and therewith explanation for some of the general characteristics of meteorites are briefly discussed. After comparison between earth and parental planet, the imagined evolution of the earth is tested by reference to the terrestrial discontinuities, mean density, moment of inertia, radioactivity, and plasticity. Rough estimates of temperatures in depth and degrees of strength of the materials in depth are deduced.

INTRODUCTION

In large measure the story of evolution, whether organic or inorganic, comes out of the earth's crust, but the deciphering is not for the geologist alone. Before he can safely translate the hieroglyphed records of rocky outcrops, he needs the Rosetta stones of geophysics and geochemistry; he needs to co-ordinate the facts of his own science with those won from the two younger sciences. In other words, the geologist should be able to assure himself that his deductions are not to be weakened when checked by the best available earth-model. Moreover, the full meaning of geophysical and geochemical data elude men of science unless their thought is guided by cosmogonic theory. Unfortunately, in spite of effort by geniuses of the past century, the theory of the earth's origin and initial condition is still in a state of flux. Nevertheless, Harold Jeffreys, one of these pioneers, has abundantly proved the value of trying to weave together well-reasoned cosmogonic theory with the results of experiments and observations in field and laboratory. His famous book and his many technical papers (*see particularly* Jeffreys, 1929; 1935) are based on the conviction that further advance in fundamental earth-science will be facilitated by intense, fact-controlled speculation about the origin and infancy of our planet. By thorough canvassing of all the possibilities of the case, geophysicist, geochemist, and geologist can expect much help in new investigations and in understanding the host of facts of their notebooks. Because Jeffreys has acted on this belief, students of the solid earth shall henceforth be deeply indebted to him; his comprehensive survey has already closed a good many blind alleys where explorers have been in danger of wasting their time and energy.

Like Daubrée, Arrhenius, Oldham, Wiechert, Holmes, Washington, and other leaders in earth science, Jeffreys favors the idea that average meteorites closely resemble the two principal kinds of average material in the earth. That this hypothesis is worthy of detailed examination and development is suggested by recent discoveries in seismology, geodesy, high-pressure experimentation, and radioactivity. The following paper attempts to show how the new data tend to support the old idea. After an orientating description of an earth-model that seems to take account of the known facts, there will be found: (1) a summary of the properties of, and problems connected with, meteorites; (2) a presentation of the most promising theory of origin for meteorite and planet; and (3) the conclusion that this theory and the associated facts of observation lead to the preferred earth-model, which, while necessarily tentative and still lacking precision in many respects, seems worthy of consideration.

Because of the uncertainties involved, the program is ambitious, but its attempting may perhaps be tolerated by those who sympathize with the astronomer's urge to advance research by building star-models. Progress is, in fact, being made by putting star-model into competition with star-model, and just as truly geology will be benefited by comparison of earth-models. To imagine the evolution of an earth composed of meteoritic material demands courage. Here and there, where known facts are insufficient for clear, unbreakable reasoning, Gordian knots have had to be cut. Apology for this is not offered; only by such handling of an intrinsically complicated subject is progress possible at all. The reader will note that there are two sets of uncertain factors, connected respectively with the suggested development of the earth and a rather definite hypothesis regarding the origin of the meteorites in our museums. It is held that, if the earth's evolution is correctly imagined, then we may expect the characteristics of meteorites to show evidence of that correctness. For this reason, as well as for the convenience of geologists who have not had time to assemble the essential facts, considerable space is here taken to outline the facts concerning meteoritic stones and irons.

Many points in the chain of arguments have been discussed with colleagues at Harvard University. The list of those who have so kindly tried to guide the writer through a maze of perplexities includes: seismologists, Miss Mary Collins and Professor L. D. Leet; physicists, Professors F. Birch and P. W. Bridgman; mineralogist, Professor H. Berman; astronomers and astrophysicists, Dr. F. G. Watson, Dr. M. C. Whipple, and Professors J. H. Menzel and H. Shapley. None of these expert authorities, however, should be held responsible for the highly speculative and hazardous attempt to give a reasoned account of history and structures for both meteorite and earth.

SOME PRINCIPAL FACTS GOVERNING CHOICE OF AN EARTH-MODEL

For a number of problems to be hereafter discussed, the earth may be regarded as a perfect sphere with the following properties:

Mean radius—6371 km.
Volume—1.083×10^{12} km^3.
Mass—5.98×10^{27} grams.
Mean density—5.52 gr/cm^3.
Moment of inertia—81.04×10^{43} gm-cm^2.
Acceleration of gravity at surface—980+ cm/sec/sec (gals).
Acceleration of gravity down to half-radius—970 gals at minimum.
Pressure at center—about 3,500,000 bars or 3,454,500 atmospheres.
Mean temperature at surface—10°C.
Mean thermal gradient at surface (continents)—28°/km.
Maximum observed temperature of emanating lava—1200+°C.

But for certain other problems relating to the suggested earth-model, especially that of the distribution of strength, the actual deviation from a spherical form must be emphasized. The sea-level or geoidal figure of the globe is approximately that of a perfect ellipsoid with polar radius of 6,356,912 meters and equatorial radius of 6,378,388 meters, the mean ellipticity of the meridian being 1: 297. The internal distribution of densities compels departure from that simple mathematical form. There are regional departures of small amplitude, giving a gentle waviness or wrinkling of the geoid with respect to the ellipsoid. And some leading geodesists report a much more important departure, of hemispherical scope and relatively great amplitude; this they have generalized under the name "triaxiality of the geoidal figure", one with an equatorial section that is supposed to be a perfect ellipse (Heiskanen, 1938; 1939; 1941a; 1941b; Daly, 1940). The more recently calculated ellipticities of this section range between 1: 46,000 and 1: 18,000.[1]

Particularly significant in the present problem are the discontinuities at depth, indicated by the seismologists. The discontinuities are of two kinds. One of the "first order" represents a level where there is a sudden change in the velocity of earthquake waves; it produces a complete break in the curve showing the relation of wave velocity to depth. The other kind, a discontinuity of the "second order", represents a level where there is a decided change in the rate at which the wave velocity changes with

[1] Jeffreys (1941a, p. 1) has recently reviewed the gravity data bearing on the form of the geoidal equator. He shows once more that this section cannot represent a perfect ellipse but seems to agree that an elliptical form is a good first approximation. In reporting a symposium on the subject, where Jeffreys' analysis was discussed, Stoneley (1942, p. 341) stated that triaxiality in the sense implied above is "genuine". W. Lambert, senior mathematician of the United States Coast and Geodetic Survey, is also of opinion (personal communication) that the figure of the earth becomes more exact if a longitude term be included in its formula. In any case we appear to be safe in assuming considerable hemispherical departures of the geoid from a simple spheroid of rotation, and therefore rather large stress-differences at great depth in the earth. Since the stress-differences are reasonably to be regarded as "permanent", there must be decided "practical" strength in the deep layers overlying the iron core of the planet. The required degree of strength is found in the "mesospheric shell" of the earth-model to be described.

increase of depth. Such a "break" gives merely a sudden change of slope in the depth-velocity curve.[2]

Commonly the changes of material at stratigraphic unconformities correspond to seismological discontinuities of the first order. Beneath the sedimentary veneers of the continental sectors of the globe is the Basement Complex, with average thickness estimated as between 10 and 20 kilometers; the Complex belongs to the "granitic layer" of Jeffreys and others. It lies on a layer of "intermediate" rock, from 20 to 25 kilometers thick and probably less felsic than granite and less mafic than gabbro. This second principal layer of the continental sectors passes downward into a third which seems to be "gabbroic" and crystalline, though its lower part may contain much crystalline peridotite.

Seismologists are not agreed as to the thickness of the third layer. Jeffreys does not recognize any essential break of material or of elasticity between the bottom of the "intermediate" layer and a level somewhere between 400 and 500 kilometers from the surface. However, Gutenberg believes that there is a decrease in the rate of acceleration of wave velocity, this change being located somewhere between the depths of 60 and 80 kilometers. For several reasons the present writer sees reason to agree in principle with Gutenberg (1927, p. 364; 1928, p. 670; 1931, p. 221; 1939, p. 314) and places the discontinuity in question at about the 70-kilometer level under each continent. Theoretically it can be assumed to be some 15 or 20 kilometers deeper in the oceanic sectors of the globe.[3]

The facts of petrology strengthen the conclusion that the earth is slowly cooling, and that through most of geological time the gabbroic layer was directly underlain by a comparatively thin, world-circling layer of vitreous basalt, too hot to crystallize in spite of the weight of the granitic and intermediate-rock layers resting upon it. However, the latest seismological and experimental data (*see* Birch and Bancroft, 1942, p. 485) seem to indicate two possibilities: either that the vitreous layer of basalt has been reduced by the earth's cooling to a thickness of less than about 5 kilometers, or else is now represented by separated remnants. The velocities of the earthquake waves at depths beginning, under the continents, not far below the 70-kilometer level seem to be appropriate for an

[2] These definitions describe ideals. In reality most of the discontinuities are transitional, through zones or thin earth-shells, and in general the change of wave velocity or of its first derivative with respect to depth is not located at any one level. However, to facilitate later discussion and computation, each break will be arbitrarily assigned to a definite level. Resulting errors in computation can hardly be important.

[3] In several earlier papers the present writer suggested that this discontinuity occurs 60 kilometers below the surface of a continent and about 80 kilometers below sea level in the deeper part of the Pacific Ocean. The geophysical evidence on this subject is far from satisfying, but in recent years the combination of seismological observations and laboratory experiments on the elasticity of igneous-rock material, both crystalline and vitreous, has forced the conclusion that the break should be located about 10 kilometers deeper down in both continental and oceanic sectors.

ultra-basic glass, which does constitute a world-circling layer. There are some indications that the top part of this lower vitreous material is peridotitic and charged with a few per cent of dissolved water. The following speculation about the evolution of a "meteoritic" earth will lead to the conception that the peridotitic phase of the vitreous layer merges downward into vitreous silicate, nearly identical with the silicatic part of the average achondritic meteorite.

It is convenient to use the inclusive name "sial" to cover the granitic and intermediate layers and the name "sima" to cover basalt and ultra-basic material, whether crystalline or vitreous.

Under the mid-Pacific and other extensive areas of deep water, the sial is either lacking altogether or is developed only as thin, regional patches. The wave velocities favor gabbroic material as probably dominant in these parts of the earth's crust. At its very bottom the crust may here be largely crystalline-peridotitic.

Below the depth of about 90 kilometers the temperature, everywhere over the planet, is thought to be too high for crystallization. According to Jeffreys the horizontal component of each earthquake wave has an approximately constant velocity at each depth greater than 100 kilometers, suggesting that each of the concentric layers of material below that level is nearly or quite homogeneous.

From about the 75-kilometer level (continental sectors) to that at the depth of about 413 kilometers the wave velocity increases notably and with fair uniformity. (*See* Tables 9 and 10.) Between the 413-kilometer and 480-kilometer levels the velocities of both the longitudinal wave and transverse wave increase with special rapidity.

The depth-velocity curves of Dahm, Repetti, Jeffreys, Wadati and Oki, and Witte show that between the 480-kilometer and 950-kilometer levels the velocity gradient is comparatively steep, as if fixed by the effect of increasing pressure on the elasticity of material whose intrinsic (chemically determined) density slowly increases with depth. The acceleration of the wave velocity diminishes with some abruptness at about the 950-kilometer level. Thereafter the rate of the acceleration does not greatly change until the 2700-kilometer level is reached. However, near this level the acceleration of the wave velocity with increase of depth is again diminished and with relative suddenness. From there to about the 2800-kilometer level the acceleration is small but positive; in the next hundred kilometers it may be actually negative.

Near the 2900-kilometer level is the well-known discontinuity of the first order, where the velocity of the longitudinal wave drops suddenly from 13.61 to 8.1 kilometers per second (Jeffreys) or from 13.7 to about 8.0 kilometers per second (Gutenberg and Richter). Here begins the iron

TABLE 1.—*Proposed earth-model: Discontinuities and shells (Continental sectors)*

Depths of discontinuities (km)	Composition between discontinuities	Temperatures °C.	Pressures (bars)*	Densities	Densities in real earth, as computed by Bullen**
0	Crystallized "granitic."	10	1	2.7	.....
15		450	4,000	2.81	.....
I.....	.....	.....	.....	.....	
15	Crystallized "intermediate."	450	4,000	2.85	.....
40		900	11,000	2.95	3.33
II.....	.....	.....	.....	.....	
40	Crystallized gabbroic and peridotitic.	900	11,000	3.00	3.33
70		1200	21,000	3.25	3.35
III.....	.....	.....	.....	.....	
70	Thin shell or "remnants" of gas-charged, vitreous basalt overlying hydrous, vitreous peridotite, which passes down into vitreous meteoritic silicate (composition achondritic).†	1200	21,000	3.05	3.35
413		2200	135,000	3.50	3.64
IV.....	.....	.....	.....	.....	
480	Crystallized meteoritic silicate (composition achondritic, passing downward into chondritic).†	2400	160,000	3.80	3.83
950		2900	350,000	4.25	4.64
V.....	.....	.....	.....	.....	
950	Crystallized meteoritic silicate with interstitial nickel-iron, which at top is 2 per cent of the mixture by volume and increases to 25 per cent at bottom.††	2900	350,000	4.35	4.64
2700		3900	1,200,000	6.3	5.54
VI.....	.....	.....	.....	.....	
2700	Crystallized meteoritic silicate with interstitial nickel-iron, which at top is 25 per cent of the mixture and increases to 35 per cent at bottom.	3900	1,200,000	6.3	5.54
2900		4000	1,350,000	6.8	5.68
VII.....	.....	.....	.....	.....	
2900	Fluid nickel-iron with occluded gas.	4000	1,350,000	9.7	9.43
5100		.....	3,000,000	12.3	.....
VIII.....	.....	.....	.....	.....	
5100	Fluid inner core, less gas-charged than shell VII.				
6371		.....	3,500,000	12.8	.....

* One bar equals 98.7 per cent of a normal atmosphere.

** *See* K. E. Bullen (1940a, p. 137; 1941, p. 164; 1940b, p. 235; 1942, p. 19).

† "Composition" here refers to the average chemical composition of achondritic and chrondritic stones.

†† That at the 950-kilometer level the full 2 per cent of interstitial nickel-iron appears with perfect suddenness is theoretically not justified; it seems better to conceive the change of composition as beginning somewhat above the level named and as gradual, though rapid. The arbitrary assumption of a perfectly sudden entry of the free metal to the extent of 2 per cent was made to ease computation of the mean density and moment of inertia of the earth-model, and the assumption was retained when Table 1 was constructed.

core, whose assumed fluidity accounts for the failure of transverse seismic waves to cross the core and also goes far toward explaining the enormous drop in the velocity of the longitudinal wave.

Finally, we note the newly discovered discontinuity at the top of the "inner core". Between the 5000-kilometer and 5200-kilometer levels the velocity of the longitudinal wave jumps from about 10.33 to 11.17 kilometers per second (Jeffreys) or from about 10.2 to 11.0 kilometers per second (Gutenberg and Richter). Thus we have apparently a discontinuity of the first order, at the depth of about 5100 kilometers (top of the "inner core"). The origin of the various discontinuities near the respective depths of 70, 450, 950, 2700, 2900, and 5100 kilometers will be discussed later.

To assist the reader in following the complex argument to follow, the essential suggestions to be made regarding the earth's internal constitution are summarized, with quantitative illustration, in Table 1. The table designates the discontinuities by Roman numerals and notes the composition, temperatures, and densities of the core and respective earth-shells, as well as estimates of the pressures supposed to be ruling at the different discontinuities. All values are of course only approximate.

Generalized, this picture of the earth's constitution includes, in succession from surface to center: (1) a strong "lithospheric" shell, from about 70 kilometers to about 85 kilometers in thickness and resting on (2) a thoroughly weak, because hot and vitreous, "asthenospheric" shell averaging nearly 350 kilometers in thickness; below which comes (3) a strong, because crystallized, "mesospheric" shell, nearly 2500 kilometers thick; this resting on a bipartite core of fluid nickel-iron with occluded carbon and hydrogen and other highly volatile gases. The radius of the core measures about 3500 kilometers.

CHARACTERISTICS OF METEORITES

GENERAL STATEMENT

Many papers describing individual meteorites have been published. Among the comparatively recent, general discussions of these foreign bodies are the books or papers by Merrill (1916, p. 28; 1920, p. 449), Farrington (1907; 1915), Prior (1916, p. 26; 1920, p. 54), Adams and Williamson (1925, p. 241), Washington (1925, p. 351), and Watson (1941).

CLASSIFICATION

Elaborate classifications have been proposed. Table 2 is an abridged form of that adopted by Prior (1923); it is a modified form of the scheme worked out by Tschermak and Brezina. The basis is mineralogical composition. The problematic tektites are excluded.

TABLE 2.—*Classification of meteorites*

I. Irons or Siderites:
  a. Nickel-poor Ataxites.
  b. Hexahedrites, in which nickel is about 7 per cent by weight.
  c. Octahedrites (nickel 7 to 14 per cent; include some Ataxites).
  d. Nickel-rich Ataxites (nickel percentage exceeds 14).

II. Stony-irons or Siderolites (both iron and stony matter in large amount):
  a. Olivine stony-irons (Pallasites).
  b. Bronzite-asmanite (tridymite) stony-irons.
  c. Bronzite-olivine stony-irons.
  d. Hypersthene-anorthite stony-irons.

III. Stones or Aerolites:
  A. Chondrites (which include more than 80 per cent of all the stones):
    a. Enstatite chondrites.
    b. Bronzite chondrites.
    c. Hypersthene chondrites.
  B. Achondrites:
    1. Calcium-poor:
      a. Enstatite achondrites.
      b. Clinobronzite-olivine achondrites.
      c. Hypersthene-olivine achondrites.
      d. Hypersthene achondrites.
      e. Olivine achondrites.
    2. Calcium-rich:
      a. Augite achondrites.
      b. Diopside-olivine achondrites.
      c. Clinohypersthene-anorthite achondrites (including Sherghottites, in which anorthite is replaced by maskelynite).
      d. Hypersthene-clinohypersthene-anorthite achondrites.

RELATIVE ABUNDANCE

Merrill (1926, p. 127) estimated the weight proportions of irons, stony-irons, and stones, recorded up to the year 1926. The respective percentages are 93, 2, and 5. The great preponderance of the irons is due to their easier recognition as extra-terrestrial bodies. The meteorites seen to fall are much more representative of the actual relative abundance. On this basis Watson (1941, p. 177; *cf.* Borgström, 1937, p. 115) estimates the total weight of the stones as somewhere between four and nine times that of the total weight of irons. Borgström gave the following proportions, by weight, for meteorites seen to fall:

	Per cent
Irons	8.1
Mesosiderites + pallasites	5.0
Chondrites	83.0
MgO-rich achondrites	0.9
CaO-rich achondrites	3.0
	100.0

CHEMICAL AND MINERALOGICAL COMPOSITION

Average chemical compositions have been computed. Column 1 of Table 3 gives the percentages derived from 318 nickel-iron meteorites listed by Farrington (1911, p. 212, 218). Column 2 gives the average for three pallasites described by Tschirwinsky, and column 2a their

average after excluding free metal. Columns 3 and 3a are the corresponding averages for two mesosiderites described by Prior (1918, p. 152, 158); columns 4 and 4a, those for 53 stony meteorites listed by Merrill (1916, p. 28). These last rather closely resemble the averages expected from the chondritic stones. Columns 5 and 5a are averages for 20 achondritic stones, the percentages having been derived from Washington's (1925, p. 362) paper. Column 6 gives the averages for four metal-free meteorites (Juvinas, Frankfort, Petersburg, and Stannern), representing the most salic type of stone.

The table illustrates a notable fact—that water is lacking in the vast majority of unweathered meteorites.

TABLE 3.—*Average chemical compositions*

	Irons	Pallasites		Mesosiderites		Chondrites		Achondrites		Most salic
		With metal	Metal-free	With metal	Metal-free	With metal	Metal-free	With metal	Metal-free	
	1	2	2a	3	3a	4	4a	5	5a	6
Fe	90.67	46.08	.....	43.49	.....	12.15	.....	1.18	.....	.....
Ni	8.50	4.76	.....	3.94	.....	1.57	.....	.33	.....	.....
Co	.59	.34	.....	.38	.....	.07	.....	.04	.....	.....
Cu	.....	.02	.....	.....	.....	.....	.....	.....	.....	.....
Mn	.....	.05	.....	.....	.....	.....	.....	.....	.....	.....
SiO_2	.....	19.21	39.37	24.13	46.20	39.24	45.52	48.93	49.70	49.53**
Al_2O_3	.....	.....	.....	5.48	10.50	2.92	3.40	6.15	6.25	11.07
Fe_2O_3	.....	.....	.....	2.08	3.98	.....	.....	.....	.....	.30
Cr_2O_3	.....	.....	.....	.39	.75	.48	.54	.45	.46	.30
FeO	.....	7.13	14.63	6.71	12.84	14.79	17.15	15.86	16.11	18.44
MnO	.....	.02	.04	.08	.16	.....	.....	.23	.23	.20
MgO	.....	22.15	45.50	7.31	14.00	22.99	26.70	18.17	18.45	9.76
CaO	.....	.....	.....	4.46	8.53	2.45	2.84	7.12	7.24	9.38
Na_2O	.....	.....	.....	.20	.39	.89	1.03	.67	.68	.63
K_2O	.....	.....	.....	.....	.....	.21	.24	.27	.27	.15
S	.04	.10	.19	1.13	2.16	1.82	2.10	.54	.55	.17
P	.17	.04	.08	.22	.49	.27*	.31*	.06	.06	.07*
C	.03	.10	.19	.....	.....	.15	.17	.....	.....	.....
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00
Sp. gr.	7.70	4.93	.....	4.50	.....	3.56	.....	.....	.....	3.17

* P_2O_5.
** Includes 0.02 of TiO_2.

Table 4 states the average mineral compositions, either as calculated modes (columns 1, 2, 2a, 3, 3a, and 6) or norms (columns 4, 4a, 5, and 5a), each column corresponding to that which bears the same number in Table 3.

TABLE 4.—*Average compositions in terms of minerals*

Mineral	Irons	Pallasites		Mesosiderites		Chondrites		Achondrites		Most salic
		With metal	Metal-free	With metal	Metal-free	With metal	Metal-free	With metal	Metal-free	
	1	2	2a	3	3a	4	4a	5	5a	6
Free metal	98.34	50.0	.....	45.0	.....	10.58	.....	1.57	.....	.....
Olivine	.....	48.0	96.0	1.5	2.7	42.31	47.34	12.82	13.03	3.0
Pyroxenes	.....	.....	.....	30.6	55.6	28.91	32.40	62.25	63.24	62.7
Anorthite	.....	.....	.....	16.4	30.0	3.34	3.73	13.23	13.44	26.9
Albite	.....	.....	.....	.....	.....	7.37	8.20	5.83	5.92	5.7*
Orthoclase	.....	.....	.....	.....	.....	1.11	1.24	1.69	1.72	.7
Troilite	.12	.3	.6	3.1	5.6	5.01	5.58	1.53	1.55	.2
Schreibersite	1.12	.2	.4	2.6	4.7	.....	.....	.40	.41	.....
Chromite	.....	.....	.....	.8	1.4	.70	.78	.68	.69	.8**
Cohenite	.42	1.5	3.0	.....	.....	.....	.....	.....	.....	.....
Apatite	.....	.....	.....	.....	.....	.67	.73	.....	.....	.....
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

* Includes 0.5% of quartz.
** Includes 0.5% of magnetite.

OCCLUDED GASES

A. W. Wright and others have identified and measured the relative proportions of, and total volumes of, the gases occluded in stones and irons. Table 5 gives calculated averages for values determined before the year 1901 (Farrington, 1901, p. 401).

TABLE 5.—*Proportions of occluded gases, by weight*

	H	CO	CO_2	N	CH_4
Average for 8 irons	63.1	20.7	8.1	7.5	0.6
Average for 6 stones	17.6	4.2	71.8	2.2	4.2

The total volumes of gas in the irons ranged from 0.97 to 47.13 times the respective volumes of solid, the average ratio being about 8 to 1. Excluding the abnormally high value of 47.13 for one of the irons, the ratio drops to 3 to 1, close to the ratio for the stones.

SOME DETAILS REGARDING THE CONSTITUENT MINERALS

General statement.—For present purposes it is not necessary to review exhaustively the mineralogy of meteorites, but a summary of significant features will be of service in the theoretical study of origin for both meteorite and earth-shells.

Free metal.—Using analyses available in 1907, Farrington (1907, p. 110) estimated the weight proportions of the four most important metals in iron meteorites. His percentages for iron, nickel, cobalt, and copper are 90, 9, 0.9, and 0.02 respectively.

Reliable analyses show that the nickel percentage ranges between 5 and 26. The measured specific gravity of the nickel-irons, as reported, ranges from about 7.0 to 8.1. Eleven nickel-rich irons (average percentage of nickel at 14 per cent) have an average specific gravity of 7.7, which contrasts with the value 7.90, expected from the actual proportions of the two metals.[4] Little of the difference can be attributed to the presence of carbon, phosphorus, and sulphur in the meteoritic material. Apparently much more important in the lowering of the theoretical density is the common fissuring of the iron meteorites. The "porosity" due to cracks is estimated at about 2.6 per cent. Explanation of this relatively great looseness of texture will be suggested later. Meantime it may be noted that, while some irons are malleable, many others are more or less friable under the hammer, as if they had been much fissured and therewith expanded.

Pyroxenes.—The most voluminous constituents of the stony meteorites are the orthorhombic enstatite, bronzite, and hypersthene, named in the order of increasing content of FeO. Also in the list of pyroxenes are clinohypersthene, augite, and diopside, the last two species, particularly the diopside, being comparatively rare. The specific gravities of pure enstatite and of the densest hypersthene in the rocks of the earth's crust are 3.18 and 3.5 respectively. The ratio of $MgSiO_3$ to $FeSiO_3$ in the densest hypersthene of the meteorites indicates for this abundant constituent a specific gravity as high as 3.6.

Olivines.—The specific gravity of pure forsterite is 3.216, and that of pure fayalite is 4.068 (Geophysical Laboratory of Washington). That of common olivine in basic rocks of the earth's crust is about 3.4, and thus lower than the more ferruginous olivines of many meteorites. Here the calculated values run from 3.5 to 3.6 or slightly more. On the other hand, 21 meteoritic olivines have measured specific gravities ranging from 3.35 to 3.42, suggesting "porosity" to the extent of about 3 per cent.

Feldspars.—Inspection of Table 4 shows that in the chondrites (the great majority of the stones) alkali-bearing feldspars (oligoclase, etc.) dominate over anorthite, while in the mesosiderites, achondrites, and

[4] The specific gravities of pure iron and pure nickel at room temperature are 7.85 and 8.2 respectively.

the most salic type of stone (*see* column 6 of the table) the relation is reversed. Explanation in terms of assumed differentiation of the various types from a common melt is not obvious, but may possibly be found in fractional crystallization during two-phase convection—a process to be later described as having operated in the planet which was disrupted to form the meteorites.

Sulphide.—Troilite, a sulphide of iron, is a widespread accessory of the stony meteorites, and it occasionally forms interstitial masses of considerable size. There is no evidence that the sulphide occurred in the original home of the meteorites on a scale comparable with that indicated by the irons or stones of our museums.[5] The specific gravity of troilite is given by Tschermak as 4.8.

Glassy phases.—Many stones contain vitreous and isotropic material in small proportion. This appears in the form of interstitial grains, irregular lenses, and narrow, irregular veinlets. The chemical composition has not been thoroughly studied. Analyses show, however, that some of the glassy material, which has been given the name "maskelynite", is anorthitic in chemical composition. Other isotropic material, forming numerous, roughly parallel plates in crystals of olivine and pyroxene, is described as glass, but here no analyses are at hand.

Graphite.—Both irons and stones carry small amounts of graphite, in the form of plates, irregular grains, and, occasionally, aggregates. Minute diamonds are reported from the Canyon Diablo (Coon Butte) iron and suspected in a few others, but the rarity of this dense form of carbon as compared with graphite is remarkable, in view of the high pressure and high temperature under which meteorites are likely to have crystallized.[6]

Negative features.—With the exception of diamond, special high-pressure phases are not found among the meteoritic minerals. Equally noteworthy is the generally total absence of hydrous minerals, in both stones and irons.

TEXTURES AND STRUCTURES

Some meteoritic irons and pallasites are fine-grained; others are coarse-grained, with individual crystals attaining diameters up to 20 centimeters or more. Evidently the crystallization of these coarser types was slow and straightforward. In contrast, most of the stony meteorites

[5] Washington (1925, p. 365) was skeptical as to the great importance of the sulphide, assigned by V. M. Goldschmidt and G. Tammann in their discussions of the earth-shells. Tschirwinsky (1927, p. 204) shares Washington's opinion.

[6] It is conceivable that diamond was relatively abundant in the depths of the planet which was disrupted to form the meteorites, and that, with the sudden relief of pressure at disruption, the diamond inverted to graphite.

are comparatively fine-grained and in mineralogical confusion, indicating rapid crystallization.

Unmatched in the rocks of the earth's crust are the chondrules of the dominant class of meteoritic stones. Although occasionally a couple of centimeters in diameter, these abundant constituents are generally of dust size, to be studied adequately only under the microscope. Many chondrules are composed of individual crystals (are monosomatic); others are of multiple constitution (polysomatic), being made of two or more crystals of the same chemical composition or else made of two or more crystals of differing composition. Characteristically pitted on their surfaces, both kinds of chondrules tend toward the spherical form. Others are somewhat elongated, and still others have evidently been broken, with separation of the resulting fragments.

Tschermak gave the following list of the chondrule minerals, in the order of their decreasing abundance: olivine, bronzite, augite, plagioclase, glass, and nickel-iron.

The rounded form has piqued the curiosity of all observers. Sorby likened the small bodies to drops of liquid falling, like a fiery rain, through gas. Many monosomatic chondrules exhibit a radial structure and in some thin sections display a radial development which is decidedly eccentric. Brauns developed such a radial-eccentric structure in crystals of sulphur, growing in a slowly cooling melt, by suddenly shaking the melt. This result has been adduced in support of the hypothesis that the rounded chondrules were made so by attrition as the corresponding crystals were forced to rub against one another and against their matrix, by plastic flow within the containing mass.

That there had been strong shearing displacement in the material before stony meteorites reached the ground was proved by Farrington, who discovered slickensided interfaces.

While opinions vary concerning the origin of chondrules, expert students agree as to the wide prevalence of brecciated and tufflike structure in the stony meteorites. The constituent minerals, whether in chondrules or matrix, are commonly cracked, splintered, and displaced. In many cases thin sections show bands of opaque material which closely resembles the pseudo-tachylites of India and South Africa—extreme forms of mylonite. Among the results of the internal disturbances are (1) remarkable friability for many stones; (2) the development of permanent "porosity" of rather surprising magnitude.

DENSITIES AND "POROSITIES"

In order to test the assumption that the earth is essentially composed of matter like that of known meteorites, a general idea of the densities

that are typical of aerolite, siderolite, and siderite is a prerequisite. To meet this need some averages have been struck, though no attempt has been made to treat all published data exhaustively.

For each group examined a considerable difference was found between the specific-gravity averages from measured values and the specific gravities calculated from the densities of the constituent silicates, metallic alloys, etc. Table 6 gives actual figures and also states the "porosity" that should correspond to each pair of differences, as well as giving analogous data for terrestrial rocks.[7]

TABLE 6.—*Average specific gravities of meteorites and terrestrial rocks*

	Measured sp. gr.	Calculated sp. gr.	Corresponding "porosity" (per cent)
45 irons with 14 per cent of nickel-iron	7.642	7.850	2.6
17 irons with 5.8 per cent of nickel-iron	7.644	7.860	2.7
11 ataxites with 14 per cent of nickel-iron	7.702	7.90	2.5
14 stones.	3.519	3.598	2.2
23 granites, quartz monzonites, granodiorites	2.691	2.703	.45
41 gabbros, norites, diabases	2.986	3.000	.5

The apparent "porosity" of meteorites comes out as five or six times that of ordinary plutonic rocks, selected for freshness.[8]

A good approximate value for the ruling density of the metal-freed part of the average stony meteorite may be got from four stones—namely, Bluff, El Nakhla, Felix, and Holbrook—containing respectively 5, 0, 3, and 5 per cent of nickel-iron. The silicate parts gave corresponding measured specific gravities of 3.46, 3.47, 3.61, and 3.39, with a mean of 3.48. If the "porosity" of these four stones were cut down by only moderate all-sided pressure, the average specific gravity would be a little higher. As a basis for calculating the density of average meteoritic silicate, when subjected to pressures of hundreds of thousands of atmospheres, a 1-atmosphere density of 3.50 will be assumed.

[7] The mode of calculating the specific gravities is described by R. A. Daly (1935, p. 659, 662, where will be found a reference to J. P. Iddings' pioneer paper relating to the method).

[8] Is it not possible that the intense fracturing of many irons has caused serious leakage of the helium which has been generated by the radioactivity of these meteorites, and thus uncertainty as to their age?

ORIGIN OF METEORITES FROM A "PARENTAL PLANET"

General considerations.—The source of meteoritic material is a live problem, still lacking detailed solution, but there seems to be no good reason to doubt that both stones and irons originated in the thorough fragmentation of a single planet, a former member of the solar system. It is possible, if not probable, that the asteroids were formed during the breakup of the same "parental planet". At least some astronomers assign this body to the now-vacant place between Mars and Jupiter, according to Bode's law. The breakup of the parental planet has been ascribed to the tidal influence of massive Jupiter, though Watson (1941, p. 41) states that collision with "a satellite of Jupiter is not an impossibility." The results of such a collision would be similar to those from tidal disintegration. He thinks that, in the case of the asteroids, disruption by explosion of the parent is "unlikely." [9] He points out (p. 25) that the total mass of the asteroids "must be less than that of the earth, probably less than that of the moon". This question of size is clearly elusive, since we have no way of telling how many fragments have been swept up and incorporated by Jupiter and the terrestrial planets.

However, it seems probable that the parental planet was somewhat smaller than Mars. To guide thought and particularly to arrive at an idea of the amount of internal pressure within such a hypothetical body, let us suppose its radius to have been 3000 kilometers and also that its iron

[9] If the parental planet had the moderate temperature corresponding to the postulate that it crystallized all the way to its center, and if the planet had no more occluded gas than the amount found in meteorites, it would be hard to assign any important share of its fragmentation to explosion. It is, however, conceivable that, when liquid, the planet carried as much as 5 per cent or more of occluded hydrogen and helium, by weight. In that case, with the retrograde boiling enforced by the later crystallization, the gas pressure would mean a high internal tension. The ultimate disruption, brought about by explosion, perhaps greatly aided by tidal strain, would cause much of the gas to escape immediately into space, and another part would leak out of the fragments of meteoritic dimensions, these fragments retaining only small amounts of gas, such as those represented in Wright's analyses. If the mantle exploded, the release of pressure on the iron core might be expected to cause the explosion of the core also. Astrophysicists might, therefore, consider the possibility of the trapping of much hydrogen and other volatile gas in the interior as the original body of gas (with initial mass perhaps several times that of the crystallized planet) condensed to liquid silicate and nickel-iron.

If we assume that meteorites are small asteroids on the fringe of the circum-solar zone where the asteroids are now common, it would be natural for us to suppose that now and then meteorites much larger than any stone or iron of the museums should have collided with the earth. If such an infallen body had the mass of Eros or even one-tenth of that of Ceres, the effect on the earth's crust would have been at least regionally catastrophic. Bombardment on that scale seems never to have affected the crust since the Basement Complex became a constituent of this earth-shell. It would, therefore, be necessary to assume that the larger fragments of the disrupted parental planet were swept up before late-Archean time. Whether space could, thus early, have been cleared of these aberrant members of the asteroid swarm (no "known" asteroid now comes within some millions of miles of the earth) is a problem for an expert in celestial mechanics.

It may be added that the impact theory of the lunar "craters" and maria does not imply any necessary reference to the asteroids. Indeed, it seems quite possible that Gilbert and others were on the right track when they assumed the masses that fell into the Moon to belong to the earth-Moon system—perhaps representing debris resulting from the forceful separation of our own planet and its satellite.

core had a radius of 1000 kilometers—a length that gives the same ratio of core to mantle, by mass, as Borgström's ratio of total weight of irons seen to fall to the total weight of stones seen to fall. The mean density of the parental planet, of its mantle, and of its core would be about 3.27, 3.6, and 8.1 respectively. Computed, rough values of gravity at the surface and of pressure at base of mantle and center of planet are given in Table 7, which also states corresponding quantities for three of the existing terrestrial planets.

TABLE 7.—*Comparative data for Earth, Venus, Mars, and a "Parental Planet"*

	Earth	Venus	Mars	"Parent"
Mean radius (km)	6371	6200	3385	3000
Volume (earth at unity)	1	0.92	0.15	0.105
Mean density	5.52	4.86	3.96	3.77
Radius of iron core (km)	3500	2900	1300	1000
Mean density of mantle	4.5	4.4	3.7	3.6
Mean density of iron core	10.7	10.5	8.4	8.1
Total mass (earth at unity)	1	0.81	0.108	0.07
Mean gravity at surface (earth at unity)	1	0.85	0.38	0.32
Pressure at base of mantle, in thousands of bars	1350	1250	300	225
Pressure at center, in thousands of bars	3500	2700	500	350

The nature and history of the parental planet would be deducible in some vital details if astronomers had been able to work out a completely satisfying theory for the solar system. Jeffreys and others have discussed hypotheses involving tidal and, alternatively, collisional separation of fractions of stellar gas, this gas having at the start an enormously high temperature, comparable with the mean temperature of a thick layer just below the photosphere of the Sun. Recently Whipple (1941, p. 237) has announced Spitzer's proof that the planets could not have been formed by a direct condensation of material taken forcibly from the Sun's surface or interior. At the bottom of the solar shell from which the planetary mass would have to come, there would be a temperature of "18 millions of degrees Fahrenheit". If suddenly torn out, the gases would immediately explode, long before they could cool by radiation. Even a mass twice that of Jupiter would not hold the gases and permit them to condense.

Whipple suggests that a solar origin might still be acceptable if it could

be shown that the solar gas was separated from the gravitational field of the Sun with sufficient slowness, but this latest reporter on the cosmogonic problem has reluctantly admitted doubt that any of the published mechanisms for planetary genesis is valid.

On the other hand, Whipple (p. 243) regards as "fairly certain" the following conclusions: (1) that each planet was once very hot, far above the temperature of melting rock; (2) that the planets were individualized through a rapid condensation of their material; and (3) that this material came from the Sun or possibly from another, visiting, star.[10]

Analogy of the earth.—To make progress in the theory of our parental planet, we can not do better than to base thought on Jeffreys' deductions in the case of the earth, though, as we shall see, it seems necessary to revise some points in the argument about the earth's own evolutionary changes. From Jeffreys' study of the earth, supplemented by the facts of meteoritic lore and by the facts known about igneous rocks, we may reasonably make the following assumptions concerning the parental planet.

(1) That it was initially gaseous, with a near-surface temperature at least as high as 3000°C., the boiling point of iron at low pressure;

(2) That the temperature of the gaseous mass increased with depth, at a rate not readily determined;

(3) That a considerable part of the original mass was lost to space, because of the volatility of the lighter gases in a weak gravitational field;[11]

(4) That radiation and initial, sudden expansion of the gas liquefied the mass at a high rate;

(5) That, during the gaseous stage, liquid drops of metallic nickel-iron sank to the center of the mass, to remain there as the nucleus of a metallic core;

(6) That somewhat later droplets of nickel-iron were joined by drops of liquid silicate, which, because of immiscibility with the metal and because of lower density, were assembled above the growing core;

(7) That liquid drops of both metal and silicate continued to fall through the thick, residual body of gas, even after a "mantle"[12], a continuous layer of dominant-silicate liquid, had formed on the core;

[10] Conceivably excessive temperature for the planetary gas was not reached because the planetary material was derived from a cool-star companion of the Sun, in collision with a third star, now lost in the depths of space.

[11] The oxygen of the original gas went into chemical combination with silicon, magnesium, iron, hydrogen, and carbon. The resulting water-gas, carbon monoxide, and carbon dioxide, being specially volatile gases, like the inert gases and hydrogen, were largely lost to space. On the other hand, some of these gases, trapped in the rapidly formed and rapidly segregated liquid, were occluded in an unoxidized state.

[12] Throughout this paper the word "mantle" is used in its logical sense as including all of the rocky material above the iron core. This definition differs from that of the seismologists, who, rather arbitrarily, separate the "continental layers", or sial, from the "mantle".

(8) That this mantling layer attained the mean composition of an "ultra-basic", multi-component rock, mixed with disseminated metal as a separate phase;

(9) That, from the moment when the mantle became complete, its surface temperature, because of radiation to outer space, fell with extreme rapidity;

(10) That the corresponding change of density induced convection currents in the outer part of the mantle, whereby the temperature there fell at a rate much greater than that expected by radiation plus simple conduction of heat from below.

Jeffreys, who throughout was considering the earth an analogous body, did not consider in detail the effect of suspended droplets of metal on the distribution of densities and therefore the possibility of thermal convection in the earth. He wrote (1929, p. 138-140):

"The heavy materials of the iron core quickly settled to the centre and stayed there. Liquids in general contract and become denser as they cool, and the matter cooled at the surface by radiation quickly sank through the hotter liquid below, thus maintaining irregular convection currents and a continual supply of heat to the surface. It is probable, though not certain that stirring by the currents would keep the whole of the rocky shell uniform in composition . . . . In a well-stirred fluid magma, while the actual temperature and the melting point both increase with depth, the melting point increases the faster. As the fluid cools, therefore, the melting point is first reached at the bottom, and solidification proceeds from the bottom upwards. Convective agitation of the fluid continues until solidification is complete; the rate of solidification is determined by the condition that the heat given out by the liquid at the bottom in solidifying and by the body of the liquid in cooling must supply the actual loss by radiation from the outside.

"The iron core hardly affects these considerations. There would always be continuity of temperature across the boundary, but the difference of density is too great for convective interchange between core and shell, though convective currents would exist within both. . . . . The core and shell would cool together until solidification began and stopped convection at the bottom of the shell. Thenceforward the core could cool only by conduction through the solid, and further loss of heat from it became unimportant."

The conclusion that solidification proceeded from the bottom upward had already been reached by L. H. Adams (1924, p. 459).

However, it seems highly probable that the convection in the silicate shell of the earth, like the convection in our parental planet, would not have been confined to the simple, throughgoing type illustrated in experiments of laboratory scale or in the atmosphere nowadays. In the present problem the scale is planetary, and the colossal weights in the earth's powerful field of gravitation must have had complicating effects. First, the lower part of the silicate shell was probably frozen, crystallized, by pressure before thermal convection could be important.[13] In the case of

[13] Superficial cooling of the liquid by radiation was relatively slow before the mantle attained full thickness. Before that stage was reached the pressure within the deeper part of the mantle had come to exceed a million bars.

the earth about half of the mantle may have been promptly frozen in this way. This means that the mantle as a whole was not stirred and made chemically uniform by convection. Secondly, while convection would be expected in the upper, smaller, part of the mantle, it was not purely thermal. The overturn of the material was occasioned not only by the thermal gradient but also by the early appearance of dense crystals at and near the surface of the radiating liquid. Thus the convection potential is thought to have been much increased by the temporary suspension of solid crystals in the superficial part of the melt. Because two phases—solid and liquid—were present in this superficial sublayer, it dove bodily, to be replaced by risen, hotter liquid. Such overturning may be distinguished as a form of "two-phase convection".

First, then, a word about pressure-freezing *in situ*. According to Jeffreys' reasoning, the conversion of the terrestrial gas into liquid was speedy, and the thickening of the silicate mantle was correspondingly rapid. Let us assume the mantle liquid which began to cover the iron core to have had a 1-bar density somewhat higher than 3.25, the 1-bar density of the average stony meteorite, liquid and devoid of free metal. The acceleration of gravity at the top of the core, already segregated, was, then as now, not far from 980 gals or the present value at the earth's surface. Hence each kilometer of mantle liquid that was accumulated added at least 325 bars to the pressure at the bottom of the mantle. The question arises as to how thick the mantle would have to become so that its basal layer was crystallized by the growing pressure at this deep level.

An approximate answer would be forthcoming from the Clapeyron equation (quoted in the following section on Temperatures), if we suppose the equation to apply to the multi-component melt, and also if we could properly assign values to the adiabatic heating, to the latent heat set free by the crystallization, and to the change of volume with that crystallization—all three quantities being affected by the new high pressure. None of these values is known from experiments conducted at such high pressure, but in a later section of this paper, dealing with discontinuities in the earth's body, some estimates of the corresponding values for the discontinuities are hazarded. Similar reasoning makes it seem probable that the basal part of the mantle was pressure-frozen before the mantle had thickened more than about 1000 kilometers, and that, when the earth's mantle was finally complete, nearly 2000 kilometers of its thickness had been crystallized by pressure, leaving the outermost thousand kilometers or so still liquid. It is thought that the mean temperature of the thick frozen shell, a temperature raised by the latent heat of the crystallization, would be not far from 2500° C. and should have

been comparatively little changed from that early epoch to the present day.

Let us now consider the second condition for cooling the still-liquid, superficial layer of the earth. Its material in droplet form fell from the primitive envelope of gas at a late stage of the evolution—namely, after nearly all the nickel-iron had been locked in the core and the thick, pressure-frozen shell of the mantle. The superficial layer would therefore be nearly homogeneous in a chemical sense and in a condition favoring deep two-phase convection.

Both Adams and Jeffreys have assumed the mantle as a whole to have been so nearly homogeneous that ordinary thermal convection would control the cooling of the radiating mantle. However, neither of these authors took sufficient account of a complication due to the effect of pressure on the viscosity of the melt. They both simplified the argument further by neglecting the fact that the silicate of the mantle is a multi-component system—not an essentially one-component system, such as that represented by dunite. Each simplification tends to obscure the most probable mode of cooling for the superficial sublayer of the mantle.

At the surface of the liquid the pressure increased by about 325 bars for each kilometer of descent; at greater depth still faster. Now, many experiments have proved that with rising pressure the viscosity of a normal liquid also rises and that at a rapidly accelerated rate. Judging by analogies from borate and organic liquids, one may assume that the viscosity characteristic of the liquid at the earth's surface (say 100 poises) would become at least a thousand-fold greater under the pressure at the depth of 50 kilometers, and at least a million-fold greater at the depth of 100 kilometers. It is manifest that the resistance to convective shearing must have been incomparably less at the surface than at great depth.

Moreover, the convective potential was specially high near the surface, because of the intense radiation at that level. And the rapid chilling had another inevitable effect. With the cooling at the surface, separate, small crystals, corresponding to the more insoluble compounds of the multi-component liquid, were individualized. These early-formed crystals tended to sink through the less-dense residual liquid, but, because of their small size and the (even low) viscosity of the liquid, the crystals sank slowly.[14] There was also a tendency for the turbulence expected in the gas-emanating planet to keep the crystals in suspension. For a time, then, a layer of specially cooled and therefore contracted liquid, charged with crystals of greater density than this liquid, would hold its place at the surface of the silicate shell. The crystal-laden layer is a two-phase

[14] The rate of sinking would vary, approximately, with the square of the diameter of the crystal.

mass liable to foundering, if disturbed ever so slightly. Disturbance there would be in ample measure. Let us imagine the threatened layer to be 50 kilometers thick and to have a mean temperature of 1500° C.; then the diving of a disrupted fraction of the layer. Because of the great dimensions of this mass, and also because of the relatively low viscosity of the surrounding liquid, the shearing resistance to the downward plunge would be small. In spite of increase of viscosity with increase of depth the mass would quickly sink below the 200-kilometer level. There would be no time for important conduction of heat into the sinking mass, which therefore arrives at the 200-kilometer level with a temperature close to 1500°. At that depth there is a pressure of about 60,000 bars—a pressure so great that the sunken mass would be rapidly crystallized (the pressure raising the temperature of crystallization by about 500°). The latent heat of crystallization would be used to raise the temperature of the now crystallized block of rock to about 1800°. This final temperature would still be about 100° below the melting temperature at the depth described.

The change of state increases the density of the sinking mass (now a solid "block") by at least 6 per cent. Hence the velocity of the sinking is greatly increased. In spite of the increasing viscosity met with, the block plunges to the depth where its own density matches that of the deeper liquid, and, because of the rapidity of descent, the block is still many hundreds of degrees cooler than this part of the liquid. The depth at which the block comes to rest would be determined by the amount of free metal suspended in the silicate melt. If with Jeffreys we assume (the highly improbable) uniformity for the mantling shell in the chemical sense, the block would sink to the top of the iron core and stay there. On the other hand, if we are right in assuming that the mantle had already been pressure-frozen from its bottom upward to about the 1000-kilometer level, such a block would come to rest at a depth no greater.

Repeated founderings of the kind result in a sublayer, composed of a multitude of solid blocks which at first were separated by interstitial liquid at much higher temperature. In the course of time this interstitial liquid was chilled and crystallized because of the invasion by the cooler masses. The latter were of course heated, but not to their own melting temperature.

If a sufficient proportion of metallic droplets, inherited from the time of the fiery rain out of the initial body of planetay gas, had settled out, the two-phase convection would go on until the whole of the mantle was crystallized. Such a condition might be expected in our parental, comparatively small planet; later on we shall find reasons for doubting that the silicate shell of a planet as large as the earth could be completely

frozen by the combination of high pressure at depth and two-phase convection higher up.

Thus, aided by an analogy furnished by the actually existing earth, we have reconstructed a parent for meteoritic stones and irons. If our reasoning has been on the right lines, we should find the speculation to be supported by observed facts learned about meteorites. If the correspondence be satisfactorily established, the steps already taken toward the fashioning of a valid earth-model shall have found preliminary justification.

The parental planet, rapidly and nearly or quite crystallized from surface to center, had a volume much smaller than that of the earth. Its orbit was located between the orbits of Mars and Jupiter. Let us assume that, soon after its solidification, the body swung in its orbit so close to Jupiter as to have been greatly distorted by the Jovian tides, though not broken into separate pieces. Such distortion would mean intense crushing, brecciation, of the constituent crystals, with relative displacements of the crystal fragments. This may have occurred more than once. After each distortion the parental planet raced away from the strong gravitational field of Jupiter and quickly recemented itself under its own gravity. Further let us assume a final approach to Jupiter, one so close that the parental planet was torn to pieces—the result being the fragmentary asteroids and meteorites of the present day.

So far we have little more than a hypothesis *ad hoc*. But the general idea may be more tolerable when it is judged by its implications. How far is the hypothesis consonant with the known characteristics of meteorites? That it can be judged with some leniency is apparent when it is realized how many facts it explains. Some of these may now be recalled.

(1) The iron meteorites must have been formed in a reducing medium. This necessity is met if, as supposed, the parental planet came from the Sun, or from another star, or from both; for in Sun and star alike the dominant gases are hydrogen and carbon gases including carbon monoxide.

(2) The gravitative separation of iron and stone was inevitable, if the parental planet passes, in succession, through gaseous and liquid stages, and if meteoritic iron is little miscible with silicate.

(3) Our hypothesis can explain the relatively fine grain of the stony meteorites; for the postulated two-phase convection led to sudden pressure-freezing at depth, where high viscosity could not fail to limit the size of crystals.

(4) The confused texture of the stones is accounted for by tidally developed shearing, crushing, and plastic flowage; perhaps also by the

collapse of the crystal meshes as the two-phase blocks sank in the liquid mantle and had their residual liquors squeezed out.[15]

(5) The grain of the iron meteorites is in many instances coarse, and for this there may be more than one reason implicit in our evolutionary scheme: (a) The crystallization of the iron core of the parental planet was not sudden, as in the case of the convectively overturned silicate of the mantle; (b) it is possible that, under the conditions of great depth, nickel-iron had a greater power of crystallization than silicate matter at the same pressure and temperature. Even during a comparatively short time large crystals form in cooling iron ingots from the blast furnace.

(6) A remarkable degree of uniformity for the dissemination of particles of nickel-iron in the stones is an expected feature if droplets of liquid iron, too small to sink rapidly in the viscous mantle, were trapped during the pressure-freezing that was incidental to convection.

(7) The old problem of the origin of chondrules may be found to furnish another test of the hypothesis. Sorby's suggestion that chondrules represent drops of "fiery rain", falling through the primitive planetary gas and then the liquid of the mantle, appears worthy of consideration. Is it not also possible that the rounded form of many chondrules is due to friction? Appropriate friction-breeding displacements are conceivable results of tidal kneading of the parental planet by Jupiter.

(8) Although crystallized, the parental planet was still hot inside, with maximum temperature probably reaching at least 3000° C. When the body was torn to pieces, the average fragment was suddenly relieved of high all-sided pressure. This should have led to equally sudden, differential expansion of the constituent minerals, with the development of permanent "porosity", much exceeding that characteristic or ordinary crystalline rocks of the earth's crust. We have seen that both stony meteorite and iron meteorite are porous to an extraordinary degree.[16]

(9) The catastrophic relief of pressure should have had another consequence: here and there minerals with relatively low temperatures of melting might be expected to have melted at the new, greatly lowered, pressure, and, because of speedy chilling by the cold of interplanetary space, to be converted into permanently enduring glass.

[15] If the tidal disruption took place during only one close approach to Jupiter, the crushing of the material of the parental planet would be less than that expected on the hypothesis of several close approaches, as suggested in the text, but conceivably the crushing and shearing would still be significant.

[16] Dr. F. G. Watson has suggested to the writer that some of the porosity of meteorites may reflect their straining while they were in rapid flight through the earth's atmosphere.

Have we not here an adequate explanation of maskelynite and of silicate chondrules which are partially or wholly vitrified?

(10) On a similar ground we can account for the injection of tongues and veins of iron into silicates of the stony meteorites. Increase of pressure raises the melting temperature of iron much more slowly than it raises the melting temperature of olivine, pyroxene, and other important silicates occurring in the stones. In this respect nickel-iron containing a small percentage of carbon would show even greater contrast with the silicates. The sudden fragmentation of the parental planet might, therefore, have been accompanied by the liquefaction of the little masses of iron which had been trapped in the silicate when this was pressure-frozen.

Melting of the crystallized iron torn out of the planetary core may well have occurred. By free radiation of heat to space, such fragments would have been quickly chilled and crystallized. They, like masses of any core metal which, possibly, had never been crystallized, should now have fine grain. In fact, many irons of the museums are fine-grained.

The foregoing analysis, sketchy as it is, seems to strengthen the grounds for the old speculation—that meteorites are disrupted fragments of a planet of the terrestrial type. That widely held idea has been tested in some new ways. How far multiple testing through comparison of logical deductions with observed facts gives affirmative results must be left to those more competent in this field of investigation. The need of their criticism is just as sincerely felt as we now proceed to the final and principal topic of this paper—the use of the general theory for planet and meteorite in constructing a "meteoritic" model of the earth.

DATA FOR CONSTRUCTING A MODEL OF THE EARTH

Descending from the thin air of more or less pure speculation, we come "down to earth" in a figurative as well as literal sense. Concerning our own planet we have important data which are lacking for other members of the solar system, and *a fortiori* for the planet assumed to be parent of the meteorites. Special facts that must control any theorizing about the earth's evolution and structure include:

(1) the mean density of the globe;

(2) its mean moment of inertia;

(3) its internal discontinuities;

(4) the composition of its surface shell;

(5) the existence of a strong crust (the lithosphere), overlying an extremely weak layer (the asthenosphere);

(6) the approximate triaxiality of the geoid, implying a considerable degree of strength in a thick layer (the mesospheric shell) below the asthenosphere;[17]

(7) the seismological evidence of a fluid condition for the terrestrial core, the material underneath the thick, strong, mesospheric layer;

(8) the apparent concentration of radioactivity near the surface of the planet;

(9) the measured thermal gradients at the surface.

COMPOSITION AND DIFFERENTIATION OF THE EARTH'S MANTLE

Table 7 illustrates the fact that the earth has a much higher proportion of nickel-iron (about one sixth by volume and three tenths by mass) than that represented in a hypothetical parent of meteorites, a parent which had a mean composition like that estimated by the use of Borgström's figures for the average of the "meteorites seen to fall" (a proportion of about one tenth by mass). This means difference of composition for the two bodies when in the gaseous state. That the ancestral compositions did vary widely among the whole group of planets in the solar system is suggested by the great range of mean densities of these planets at the present time.

The table further shows that, relatively to the earth, the parental planet had a smaller gravity at the surface and hence a much smaller internal pressure. A smaller pressure within a liquid planet meant a lower viscosity for the liquid at a given depth below the surface. Assuming a logarithmic law (natural logarithms) for this relation, the multiplication of pressure by 2, 5, and 10 times would cause a 7-fold, 150-fold, and 22,000-fold increase of viscosity. Thus, other things being equal, settling out of iron droplets from the liquid silicate mantle of the parental planet should have been much more speedy than similar differentiation in the liquid earth. If for no other reason, convection would chill the smaller body at a faster rate and bring about the early, complete crystallization of its mantle. However, one may well doubt that in the much larger earth convection should induce complete crystallization of its mantle.

If the material of the earth's mantle were chemically homogeneous and had a single, definite melting point, pure thermal convection, operating in crystal-free liquid, might solidify the whole of the mantle, as Jeffreys imagined in the case of a dunitic mantle, but the assumption can hardly be justified when the liquid is supposed to have the composition of meteoritic silicate. For such a liquid is multi-component,

[17] For a more detailed description of these earth-shells, *see* Daly (1940, p. 13, 355, 404).

crystallizing through a temperature interval of at least 150°. For this reason convection in the liquid "meteoritic" mantle of the earth would cease long before the whole of the silicate shell could be crystallized by such convection. During the partial crystallization at the surface, and also during the more complete pressure-freezing at great depth, the crystals coming out of solution are more mafic (richer in magnesia and iron) than the original liquid, and the residual liquor is more salic (richer in silica, alumina, and alkalies) than the original liquid. Because of its lower density the residual liquor, even though mixing with the unchanged liquid above, tends to rise toward the top of the mantle. Is it not conceivable that this gravitative sorting of material went so far as ultimately to give a layer of molten granite at the surface? Such a layer would be unsinkable. Convection would be enormously slowed down, and by continued, rapid radiation of heat to outer space the mantle would be endowed with a crust of crystallized granite, also so "light" as to be unsinkable. Beneath the true crust there is thereby imprisoned a layer of molten (vitreous) silicate which had a mild increase of density from above downward.

VITREOUS LAYER (ASTHENOSPHERE)

The vitreous layer as a whole was likely to remain long in a superheated condition, for this viscous liquid had been pushed up from the highly heated depths during the rain of foundering, two-phase blocks. Because the crust and mesospheric shell thickened with great slowness, the vitreous layer, somewhat thinner than at first, should exist in the earth today. If so, we should expect discontinuities at top and bottom of this layer. Are they actually represented by the Gutenberg discontinuity, reported at a depth of 60 to 80 kilometers, and the "20-degree" discontinuity, reported at a depth between 400 kilometers and 500 kilometers? (*See* the breaks III and IV in Table 1.)

TEMPERATURE OF THE CORE

If all the planets of the terrestrial type were initially gaseous and of meteoritic composition, they should have developed different temperatures, by self-compression; in particular the earth, from the beginning, should have been hotter than the parent of our meteorites. Let us take a preliminary glance at this important problem, which farther on will have more detailed treatment.

By hypothesis the temperature of the terrestrial gas, when it began to evolve stable droplets of liquid iron, averaged at least 3000° C., the boiling point of iron. At the center of this ancestral gas the temperature was much higher. The core was largely segregated during

the gaseous stage and well before the silicate mantle attained full volume. The temperature of the core was ultimately raised by compression under the heavy mantle with its pressure approaching 1,400,000 bars. On a following page will be found the suggestion that the core, now in nearly complete thermal isolation, has at its top a temperature not far from 4000°; the temperature at its center should be considerably higher, but, partly because of our ignorance as to the extent of gravitative differentiation in the core, there is no good basis for estimating that excess.

A COMPARISON

On the other hand, the metallic core of the much smaller parental planet was far less heated by compression; it presumably contained a smaller proportion of occluded gas; and it was chilled against a mantle which had been specially cooled by two-phase convection. For all three reasons it is assumed that at least the upper and greater part of this core was crystallized soon after the overlying mantle reached full thickness.

Thus, theoretical considerations indicate contrast. Because the pressure at the base of the mantle in the parental planet was relatively small (only about one-sixth of that at the base of the terrestrial mantle; *see* Table 7), direct pressure-freezing of any part of the thinner mantle would not be expected. As we have just seen, however, two-phase convection, perhaps aided by ordinary thermal convection, was likely to be speedier than in the case of the earth, and to cause complete crystallization of the thinner mantle. Unlike the parental planet, the earth was early endowed with: a vitreous layer within the silicate mantle, a layer which during geological time has lost about half its thickness; a higher proportion of free metal, trapped in the pressure-frozen part of the mantle; and a still-existing core of fluid nickel-iron.

DISCONTINUITIES

The discontinuities found in the earth's body by the seismologist form a kind of mathematical skeleton for our "meteoritic earth-model". If the skeleton is to be of service in geology, we must try to put some flesh on it; particularly we need information regarding the state of the terrestrial material and at least approximate values for density, density-determined pressure, and temperature at all depths. In a general way outside limits may be assigned to each of these sets of values, but within those limits we can do little more than guess at actualities, unless we find guidance in one or more fields of investigation other than seismology. If, specifically, we recognize true homology between the earth and a

planetary parent for meteorites, the range of uncertainty concerning each of the desired values is decidedly narrowed. Some of the clues so provided are now to be recalled, even at the risk of repetition of statement.

(1) The meteoritic irons have density and computed temperatures of melting of the order demanded if the earth's core is free metal in an effectively fluid state. The irons contain gas and carbon in varying amounts, thus permitting one to speculate about differential effects of these accessory substances on both density and degree of fluidity within the core.

(2) The pallasites and allied siderolites suggest an upper limit of nearly 50 per cent by weight for the free metal in the basal phase of the mantle.

(3) A few of the stony meteorites are free from the nickel-iron alloy and may be described as assemblages of silicates, analogous to wehrlites and some other peridotites, though richer in ferrous iron than these, and at the same time charged with accessory feldspar and sulphide of iron. This material, 100 per cent "stony", is of the kind expected near the surface of a "meteoritic" planet in which the immiscible droplets of liquid iron had had time to settle out, toward the center—the process favored by Jeffreys.

(4) In such a purely stony layer, affected by surface cooling for a billion years or more, there should be a relatively steep thermal gradient, which would tend to cut down the rate of decrease of compressibility, and therefore wave velocity, as depth increases. However, judging from measured surface gradients of temperature, that influence should be much more than offset by pressure, rapidly rising with increase of depth. Hence, within the superficial stony layer the wave velocity should be found to increase notably with the depth. The depth velocity curves of Jeffreys (*see* Tables 9 and 10), Gutenberg and Richter, and others meet this expectation for a superficial layer about 950 kilometers in thickness. Jeffreys has shown that at this depth there has been little cooling since the earth became stably crusted. Here, then, the thermal gradient is low, and yet here the acceleration of wave velocity begins to drop in value as depth increases. We shall soon see that this change also is an anticipated feature of our evolutionary scheme for a "meteoritic" earth.

In general, the discontinuities (levels or zones) occur where, with increase of depth, the elastic properties of the terrestrial material change at specially high rates. These relatively sudden changes in elasticity correspond to changes in chemical composition, to changes of "state" (liquid to solid or vice versa), or to both changes of composition and "state" simultaneously. To translate the more or less abrupt variations of wave velocity into variations in the material, three equations are used.

Let V_p represent the velocity of the longitudinal or compressional wave; V_s, the velocity of the transverse or shear wave; K, the bulk modulus of the material (reciprocal of its compressibility, β); R, its rigidity; and D, its density. Then, for a homogeneous solid,

$$V_p = \sqrt{\frac{K + 4/3\, R}{D}} \tag{1}$$

and

$$V_s = \sqrt{\frac{R}{D}} \tag{2},$$

In the case of a homogeneous liquid

$$V_p = \sqrt{\frac{K}{D}} \tag{3},$$

and the shear wave is not transmitted at all.

Adams and Williamson have given equation (1) the more convenient form:

$$V_p = \frac{x}{\sqrt{10^6 \beta D}} \tag{4},$$

where V_p is in kilometers per second, and x represents a quantity whose value depends on Poisson's ratio for the solid. Data from Jeffreys and from Gutenberg and Richter give (Table 8) values of Poisson's ratio and inferentially the corresponding values of x, at depths which are of particular interest as we begin a brief discussion of the location and meaning of the discontinuities in the earth.

TABLE 8.—*Data for calculating compressibilities at various depths*

Depth (km)	Poisson's ratio		x	
	Jeffreys	Gutenberg and Richter	J.	G. and R.
100	.27	.27	13.13	13.13
400	.28	.30	12.99	12.71
500	.28	.30	12.71	12.71
1000	.28	.28	12.99	12.99
2500	.30	.30	12.71	12.71
2700	.30	.31	12.71	12.57
2900	.30	.31	12.71	12.57

Jeffreys' values of wave velocity at depth are founded on particularly abundant observations and are here accepted as the basis for locating discontinuities below the depth of 75 kilometers. Column 3 of Table 9 gives the velocity of the longitudinal wave running at the respective levels

named in column 1. Column 4 gives the difference of velocity for each of the successive pairs of levels; column 5, these differences expressed as the rate of increase of velocity (for the last pair a decrease) per 100 kilometers of increase of depth. Column 6 gives the rates of column 5,

TABLE 9.—*Accelerations of the longitudinal wave with increase of depth*

1	2	3	4	5	6
Range of depth (km).	Difference of depth in range (km).	Range of Vp (km/sec).	Difference of Vp for top and bottom levels (km/sec).	Rate of change of Vp per 100 km of depth.	Rates of col. 5, expressed in percentages.
33– 53	20	7.75– 7.80	.05	.25	3.2
53– 114	61	7.80– 7.99	.19	.31	4.0
114– 220	106	7.99– 8.30	.31	.29	3.7
220– 251	31	8.30– 8.39	.09	.29	3.5
251– 280	29	8.39– 8.51	.12	.43	4.9
280– 331	51	8.51– 8.69	.18	.36	4.1
331– 361	30	8.69– 8.79	.10	.33	3.8
361– 402	41	8.79– 8.91	.12	.29	3.3
402– 420	18	8.91– 8.96	.05	.28	3.1
420– 463	43	8.96– 9.42	.46	1.07	12.0
463– 524	61	9.42– 9.82	.40	.66	7.0
524– 551	27	9.82– 9.96	.14	.52	5.3
551– 580	29	9.96–10.11	.15	.52	5.2
580– 612	32	10.11–10.26	.15	.47	4.6
612– 637	25	10.26–10.38	.12	.48	4.7
637– 662	25	10.38–10.49	.11	.44	4.3
662– 784	122	10.49–10.97	.48	.39	3.8
784– 888	104	10.97–11.23	.26	.25	2.3
888– 985	97	11.23–11.38	.15	.15	1.4
985–1093	108	11.38–11.55	.17	.16	1.4
1093–1234	141	11.55–11.75	.20	.14	1.2
1234–1404	170	11.75–11.99	.24	.14	1.2
1404–1583	179	11.99–12.24	.25	.14	1.1
1583–1771	188	12.24–12.49	.25	.13	1.08
1771–1949	178	12.49–12.72	.23	.13	1.03
1949–2138	189	12.72–12.95	.23	.12	.96
2138–2353	215	12.95–13.20	.25	.12	.90
2353–2548	195	13.20–13.42	.22	.11	.85
2548–2730	182	13.42–13.60	.18	.10	.74
2730–2823	93	13.60–13.65	.05	.05	.4
2823–2873	50	13.65–13.65	.00	.00	.0
2873–2938	65	13.65–13.61	−.04	−.06	−.05

now expressed as percentages of the smaller velocity, in column 3, for the respective pairs (Adams and Williamson, 1923, p. 526; Gutenberg and Richter, 1939, p. 134; Jeffreys, 1939, p. 510).

Table 10 contains similar data for the transverse wave.

The "granitic", "intermediate", and "gabbroic" sublayers of the continental crust have been explained as differentiates of the primitive liquid, meteoritic silicate, though it is not objectively clear as to why that

differentiation should have produced the discontinuities at the base of the "granitic" sublayer and at the base of the "intermediate" sublayer. That double problem will not here be undertaken. The six still deeper discontinuities, demanding conditions more directly associated with a mete-

TABLE 10.—*Accelerations of the transverse wave with increase of depth*

1	2	3	4
Range of depth (km)	Range of V_S (km/sec)	Difference of V_S for top and bottom levels (km/sec)	Rate of change of V_S per 100 km of depth, expressed in percentages
33– 96	4.35–4.44	.09	3.3
96– 160	4.44–4.53	.09	3.2
160– 223	4.53–4.64	.11	3.7
223– 287	4.64–4.74	.10	3.4
287– 350	4.74–4.85	.11	3.7
350– 413	4.85–4.96	.11	3.6
413– 477	4.96–5.24	.28	8.8
477– 540	5.24–5.48	.24	7.3
540– 603	5.48–5.65	.17	4.9
603– 667	5.65–5.84	.19	5.4
667– 730	5.84–6.01	.17	4.6
730– 794	6.01–6.14	.13	3.4
794– 920	6.14–6.29	.15	1.9
920–1047	6.29–6.39	.10	1.3
1047–1174	6.39–6.48	.09	1.1
1174–1301	6.48–6.56	.08	1.0
1301–1427	6.56–6.64	.08	1.0
1427–1554	6.64–6.70	.06	0.7
1554–1681	6.70–6.77	.07	.8
1681–1808	6.77–6.83	.06	.7
1808–1934	6.83–6.90	.07	.8
1934–2061	6.90–6.95	.05	.6
2061–2188	6.95–7.01	.06	.7
2188–2315	7.01–7.07	.06	.7
2315–2441	7.07–7.14	.07	.8
2441–2568	7.14–7.20	.06	.7
2568–2695	7.20–7.26	.06	.7
2695–2822	7.26–7.31	.05	.5
2822–2895	7.31–7.30	−.01	−.2

oritic composition for the planet, should furnish an important, multiple test of our earth-model.[18]

The meaning of the change in the depth-velocity curve, found by Gutenberg at the base of the "gabbroic" sublayer, was a topic introduced near the beginning of this paper; the "break" is thought to be of the second order and to correspond to a transitional zone between the "gabbroic"

[18] It will be recalled that in general the discontinuities represent changes extending through zones or thin earth-shells. However, to simplify reference to them and to facilitate certain calculations, each "break" will be regarded as located at a single, definite level.

material and the underlying peridotitic material.[19] The vitreous state, characteristic of the asthenosphere, is supposed to be fully established at —70 kilometers under the continents and at —85 to —90 kilometers under the deep ocean. (The negative sign indicates depth.)

The remaining discontinuities, in the order of increasing depth, will now be considered.

While in each of the tables 9 and 10 the acceleration of wave velocity with increase of depth is irregular, the rates of change themselves show comparatively sudden changes at five loci, all below the depth of 100 kilometers.

The first break occurs between —400 kilometers and —500 kilometers. As already suggested, this discontinuity may represent a transition from the vitreous asthenosphere to an underlying, crystallized layer, which, as the "mesospheric" shell, extends down to the iron core.[20] If this is true, the discontinuity in question should be of the second order, because the silicate mantle is a multi-component system, melting through a range of 150° C. or more. With the expected downward increase in the proportion of interstitial glass there should be a gradual acceleration of wave velocity—not a sharp increase of velocity—as the depth increases. The total change of velocity between —400 kilometers and —500 kilometers accords with this reasoning. Seismologists call this second-order break the "20-degree discontinuity".

The second break, also of the second order, appears between —900 kilometers and —1100 kilometers. Within that zone the rate of increase of wave velocity with depth rapidly drops. The rate remains small all the way down to the base of the mantle. A discontinuity of the kind is indicated by the depth-velocity curves of Jeffreys, Dahm, Gutenberg and Richter, Witte, and Repetti—at the respective depths of about 930, 930, 1000, 925, and 970 kilometers. (*See* the tables assembled by J. B. Macelwane *in* Gutenberg, 1939, Chapter X.) The round-number average of 950 kilometers for the depth will be assumed.

This discontinuity can be understood if, near the 950-kilometer level,

[19] Both gabbro and peridotite are multi-component and melt through considerable intervals of temperature; hence the contact of crust and vitreous asthenosphere cannot be sharp. Wave velocity should change but little in the transitional zone of contact. Gutenberg (1939, p. 314) finds that "at a depth of 50 or 60 kilometers the velocity ceases to increase with depth and that at slightly greater depth it even decreases with increasing depth. . . . The total decrease in velocity cannot be large". From about the 80-kilometer level downward the acceleration of velocity is once more positive.

[20] *Cf.* Daly (1940, p. 13, 355, 397 ff., 404). In that book (p. 409) the writer suggested that the triaxiality of the earth, involving ellipticity for the geoidal equator, is due to a still greater ellipticity of the mesospheric shell at the equator. Conceivably a valid explanation for such departure from circularity may be forthcoming if it were ever proved that the material of the Moon was torn out of the earth after the greater part of the mesospheric shell had become crystallized and before the earth was endowed with a stable crust. But it is clear that the problem remains in the realm of nearly pure speculation.

there is the rather sudden appearance of nickel-iron (*note* its high density and *see* equation No. 1 above), interstitial in the mantle silicate.[21] The acceleration of wave velocity with added depth keeps a fairly uniform rate from —950 kilometers to —2700 kilometers, as if the addition of free metal is at an approximately uniform rate, from the higher level to the lower. Such increase of free metal, trapped during the pressure-freezing of the lower two-thirds of the mantle, is an expected feature of our evolutionary hypothesis.

The third discontinuity occurs at the estimated depth of 2700 kilometers.[22] The depth-velocity curves of Jeffreys and Dahm were used in arriving at that figure. Both curves show, near the stated level, a sudden drop in the rate of acceleration of wave velocity; between —2800 kilometers and —2900 kilometers the rate seems actually to change sign. The diminution of rate suggests a correspondingly rapid increase in the amount of nickel-iron, the ratio of metal to silicate itself also increasing rapidly to the bottom of the mantle. *See* Table 1.

For the levels just above and below —2900 kilometers, the contact of mantle and core, Jeffreys assigns to the longitudinal wave the respective velocities of 13.61 and 8.1 kilometers per second. At the same depth Gutenberg and Richter found the drop in velocity to be from 13.7 to about 8.0 kilometers per second. This fourth discontinuity, the most impressive of all, is in large part explained by the assumption that the break is one between silicate (albeit iron-charged) in the crystallized state and nickel-iron in the fluid state. However, another part of the sudden drop in velocity must, apparently, have a different cause.

The reason for the last statement is based on the amount of change in V_p at the crossing of the discontinuity. At the top of the fluid core the density is taken to be 9.7; that at the bottom of the mantle at 6.8. (*See* Table 1.) Assuming the core to be fluid, V_p within it is equal to the square root of K/D, where K is the bulk modulus. With Jeffreys' value for V_p, just below the discontinuity, namely 8.1 kilometers per second, the compressibility (β) at the top of the core is found to be 0.157×16^{-6}. For the crystallized silicate-iron at the base of the mantle β is calculated from the equation

$$V_p = \frac{x}{\sqrt{10^6 \beta D}},$$

x being taken at 12.71 (*see* Table 8) and V_p at 13.61 kilometers per second. Here β comes out at 0.128×10^{-6}. If Bullen's computed value

[21] It is assumed that, with respect to wave velocity between the 950-kilometer and 2700-kilometer levels, the effect of the growth of pressure outweighs the opposite-sign effect wrought by increase in the proportion of free metal.

[22] This "break" is not generally recognized by seismologists.

of D, namely 5.68, be used, β comes out at 0.154×10^{-6}. In either case β is smaller than the β computed for the nickel-iron just below, and this in spite of some degree of probability that β for the dominantly pyroxene-olivine material above should be somewhat greater than β for pure, fluid nickel-iron below the discontinuity.[23]

The apparent excess of compressibility for the core material is another problem for speculation. Is it possible that the compressibility of the upper part of the iron core, and therefore the wave velocity in it, may be affected by the occlusion of much gas with compressibility higher than that of the metal freed from gas? It is natural to suppose that such volatile matter, initially occluded in the core, would have diffused upward and become more or less concentrated near the top of the core.

The latter hypothesis might also help us to account for the fifth and last discontinuity of our list. Jeffreys located it near —5100 kilometers and described it as the top of the "inner core" of the earth. Between the 5030-kilometer and 5170-kilometer levels there is a net, strongly marked increase of V_p by about 0.84 kilometers per second.[24] Gutenberg and Richter (Gutenberg, 1939, p. 356) discovered a similar break at about the same depth. Is this discontinuity due in part to a rapid decrease of occluded hydrogen and other gases as the depth increases past the 5100-kilometer level?

MEAN DENSITY AND MOMENT OF INERTIA

Manifestly the distribution of free metal, as well as the compression suffered by the terrestrial materials at depth, must cause downward increase of density through most of the mantle. To such increase of density a limit is fixed by two quantities: the mean density of the earth and its moment of inertia. Each has been closely ascertained. An indefinitely large number of distributions of free metal in the silicate mantle would give those quantities. Yet, guided by the ratios of iron to silicate in meteorites, the writer has made many different assumptions as to the possible variation of density in a "meteoritic" earth and then calculated what mean density and moment of inertia would be. It was found that, if there is no free nickel-iron above the 950-kilometer level, the actual law for the distribution of the metal could not differ widely from that embodied in Table 1. The computations were of course

[23] Direct experimental evidence on the relative compressibilities is of course out of the question.

[24] Jeffreys' (1939, p. 602) illustration shows a sharp downward kink in the depth-velocity curve in the region just above the 5100-kilometer level. K. E. Bullen (1941, p. 165) also describes this curious interruption in the course of the curve. Apparently in neither case has the author published satisfactory proof of such a local reversal of the law of increase of wave velocity with increase of depth. However, the very rapid net increase in velocity near the 5100-kilometer level seems to be established.

founded also on reasoned values for the compressibility of both silicate and free metal. What those values should be under the extreme conditions of pressure and temperature is a difficult problem which obviously needs discussion.

COMPRESSIBILITIES AND DENSITIES WITHIN THE MANTLE

Thanks to the pioneer work at the Carnegie Geophysical Laboratory of Washington and at the Harvard University laboratories of Birch and Bridgman, we have a highly accurate idea of the compressibilities of the standard types of igneous rocks at all pressures up to 10,000 bars. In a good number of cases the Harvard investigators have measured the modifying effect of temperatures that reached a maximum of 500° C. Of present interest are the results for the ultra-mafic rocks, those closest to the average stony meteorite. Here the dominating constituents are orthopyroxenes and olivine. Both kinds of crystals are richer in iron silicate than the accessible common pyroxenes and olivines of the earth's crust, but from the compressibilities of the latter the compressibility of the meteoritic silicate, where not affected by differentiation, can be estimated.

The initial 1-bar density of the crystallized silicate, at room temperature, is assumed to be 3.50. Down to the depth of a few hundreds of kilometers (the zone of comparatively steep thermal gradient), pressure and temperature almost completely annul each other's effect on

TABLE 11.—*Compressibilities and densities of meteoritic silicate if crystalline and at the pressures and temperatures existing in the earth's mantle*

1	2	3	4	
Depth (km)	Pressure (bars)	Density	Compressibilities, per bar x 10^6	
			(a) Met. silicate	(b) Earth-model
0	1	3.50	1.00	1.98
35	10,000	3.53	.92	.92
85	25,000	3.57	.82	.848
160	50,000	3.64	.70	.776
315	100,000	3.78	.60	.675
625	200,000	4.00	.45	.390
925	300,000	4.19	.35	.297
1,150	400,000	4.33	.30	.258
1,590	600,000	4.58	.25	.216
1,975	800,000	4.80	.21	.183
2,340	1,000,000	5.00	.18	.157
2,670	1,200,000	5.18	.16	.139
2,900+	1,400,000	5.34	.14	.128

density. At greater depth, where the thermal gradient is much gentler, the pressure effect dominates. The actual compressibility of the material at —2900 kilometers (with density taken at 6.8) has been found to be 0.128×10^{-6} per bar. That of the silicate alone would presumably be a little higher—at about 0.14×10^{-6}.

From these data the writer has dared to construct columns 3 and 4(a) of Table 11, giving approximate densities and compressibilities for the meteoritic silicate of the mantle, when arbitrarily supposed to be undifferentiated and devoid of free metal. With the aid of Table 11, and allowing for magmatic differentiation in the mantle and also for the appearance of free metal at the 950-kilometer level, approximate densities at various levels within the meteoritic earth-model have been computed and entered in Table 1. Using these densities and Jeffreys' values for the wave velocities at the respective levels, the compressibilities expected in the earth-model were found and are stated in column 4(b) of Table 11.

TEMPERATURES

The range and distribution of temperatures in the globe constitute a problem as vital to the geologist as it is baffling to the geophysicist. Extrapolation from measured thermal gradients at the surface is unsafe beyond the depth of a few scores of kilometers. For deeper levels it is a case of guessing, guided by any promising analogies. Guesses published within the last 40 years assign to the center temperatures as low as 2000° and as high as 20,000° C. More definite ideas can hardly be reached without help from a general theory of the earth's origin and early history. Can the evolutionary scheme leading to our earth-model give some measure of guidance?

According to the model there are three levels where the terrestrial material changes state, and each change implies a rather definite temperature. One of the levels is at the base of the lithospheric shell; a second at the base of the asthenospheric shell; and the third at the base of the mesospheric shell. In the first case we shall, for the present purpose, ignore any basaltic glass that may be present there and shall regard the change as from crystalline peridotite above to vitreous peridotite below. At the second level the change is from vitreous meteoritic silicate above to crystalline meteoritic silicate below. At these two levels the materials are at their respective melting "points"—the fact that the melting must be through an interval of temperature and through a finite depth being ignored. The same arbitrary treatment will be adopted for the level where the change is from crystallized pallasitic material to "fluid," impure nickel-iron at the top of the earth's core.

Here the temperature cannot surpass that of the liquids for the pallasitic material at the given depth.

The guessing at the three temperatures will be helped by the use of the well-known Clapeyron equation:

$$dT/dp = 0.024 \times T_0 \times (V-v)/L,$$

where T_0 is the absolute melting point at zero pressure; $(V\text{-}v)$ is the difference of specific volumes of the liquid (V) and crystalline (v) phases; L is the latent heat; and dT/dp is the rate of change of the melting point per bar.

Approximate values of $(V\text{-}v)$ and L for the peridotite, for meteoritic silicate, and for pure iron are given in Table 12.

TABLE 12.—*Data for applying the Clapeyron equation (initial pressure, 1 bar)*

	Temperature	Specific gravity	Specific volume	$V-v$	L (gram calories)	dT/dp (per bar)
Ordinary peridotite*				0.0206	100	0.009
Crystalline	0°	3.3	0.3030			
Crystalline	1400° (m. pt.)	3.14	.3184			
Liquid	1400	2.95	.3390			
Meteoritic silicate*				0.0258	100	0.0104
Crystalline	0°	3.5	.2857			
Crystalline	1400° (m. pt.)	3.35	.2985			
Liquid	1400	3.08	.3243			
Pure iron				0.0055	65	0.00367
Crystalline	0°	7.85	.1274			
Crystalline	1535° (m. pt.)	7.35	.1360			
Liquid	1535	7.067	.1415			

* For the ordinary peridotite and meteoritic silicate it is assumed that the decreases of density on melting are respectively 6 and 8 per cent.

First we shall estimate the temperature at the 70-kilometer level, where the pressure is about 21,000 bars and thus within the reach of existing high-pressure apparatus. At this comparatively low pressure, $(V\text{-}v)$ and L are not likely to differ widely from their values at atmospheric pressure. With T_0 at 1400° C., the Clapeyron equation gives a melting temperature somewhat below 1575°, on the assumptions that the material here is peridotitic, that no volatile matter is present, and that pressure slightly decreases $(V\text{-}v)$ from the value shown in Table 12, while slightly increasing L (obeying Bridgman's general rules for normal substances).

The first of the three assumptions needs a word of explanation. Although, as remarked near the beginning of this paper, the material just below the true crust seems to have been basaltic throughout most of post-

Archean time, recent correlation of seismic-wave velocities with measurements of the elastic constants of rocks at high pressure and temperature indicates that the asthenospheric material, where continuous around the globe, is more femic than basaltic glass. Perhaps, too, the base of the crust now includes much of this more femic material in the crystallized state. Hess (1937, p. 75; 1938, p. 83) has compiled the evidence that peridotite, richly charged with water and other volatiles, was injected into the roots of major mountain chains of relatively recent, Tertiary origin. This discovery seems to warrant the hypothesis that at or near its top the existing asthenosphere is a world-circling, somewhat hydrous, highly magnesian melt. The proportion of volatiles in solution may be supposed to diminish, probably at a high rate, with increase of depth in the asthenosphere. It seems also reasonable to assume that within the melt there is a simultaneous slow increase of intrinsic (chemically determined) density of the silicatic matter as the depth increases. Even if extremely small, this latter change would prevent pure thermal convection in the vitreous layer, where, therefore, a steep gradient can persist without endangering the crystallinity at the base of the crust.

If the top of the asthenosphere carries from 5 to 10 per cent of volatiles, the liquidus is likely to be several hundreds of degrees lower than 1575°.[25]

Combining all the effects of pressure, temperature, and content of volatiles, a fair estimate of the temperature at —70 kilometers is 1200°. This is decidedly lower than the 1330° which the writer (assuming the effect of dissolved volatiles as negligible and also ignoring the effects of pressure and temperature on $(V\text{-}v)$ and L) formerly assigned to the 60-kilometer level.

Since the theoretical thermal gradient is steepened but slowly in passing upward from the base of the crust, the temperatures at the 15-kilometer and 25-kilometer discontinuities (continental sectors) should be close to those given in Table 1.

(2) The second of the zones which are characterized by a change of state is the 20-degree discontinuity, between the 400-kilometer and 500-kilometer levels. It is a contact of transition between the vitreous asthenospheric shell and the crystallized mesospheric shell. In order to use the Clapeyron equation the break is assumed to be sharp and located at —450 kilometers, where the pressure is about 150,000 bars, and the temperature far above 1000°. What effects these conditions have on the $(V\text{-}v)$ and L of any substance have never been determined by experiment.

[25] This judgment is based on the results of Goranson's experiments (1931, p. 481; 1938, p. 271) with water-saturated albite and granite.

Here we must think in terms of analogies, derived chiefly from Bridgman's experiments on organic liquids, carbon disulphide, and the alkali metals, at pressures generally well below 50,000 bars and at small ranges of temperature. From his experiments it seems safe to credit the above-mentioned rules connecting $(V\text{-}v)$ and L with great changes of both pressure and temperature. With the rules in mind the present writer has done some guessing.

According to our earth-model, the material at the 450-kilometer level is silicate (chemically the average achondrite), for which the low-pressure values of $(V\text{-}v)$ and L are estimated from the 1-bar values experimentally determined for olivines, pyroxenes, basic plagioclase, and troilite. Those estimated values are stated in the first two columns of Table 13. For the silicate under the conditions of the 450-kilometer level, various hypothetical values of $(V\text{-}v)$ and L are given in the other four columns of the table, where corresponding values for dT/dp and temperature of melting are also entered.

TABLE 13.—*Data bearing on the temperature (T) at the 450-kilometer level*

	At 1 bar	At 150,000 bars				
$V\text{-}v$	0.0258	0.0258	0.020	0.015	0.015	0.010
L	100	100	110	110	120	150
dT/dp	.....	.0104	0.0073	0.0055	0.0050	0.0027
T °C.	1400	2960	2495	2221	2156	1802

From the table it seems probable that T lies between 1800° and 2500° and nearer 2500° than to the lower extreme value. A reasonable guess at the actual temperature of the 450-kilometer level is 2200°, the value entered in Table 1.

(3) In a similar way the melting temperature of the meteoritic silicate at the 2900-kilometer discontinuity has been guessed. Respective values of $(V\text{-}v)$ and L were assumed to be 0.015 and 120; 0.10 and 120; 0.10 and 150; 0.10 and 200; and 0.05 and 200. The corresponding values of T, computed from the Clapeyron equation, are respectively 8175°, 5917°, 5014°, 4111°, and 2756°.

The actual temperature at −2900 kilometers may be somewhat lower than the melting "point" of the silicatic material at the discontinuity. According to our evolutionary hypothesis, this material was initially at the temperature of the boiling "point" at the pressure of the thick shell of gas out of which the droplets of nickel-iron and silicate were falling, due account being taken of adiabatic heating of the droplets. The composite liquid was further heated by conduction from the still hotter

core and, during the long stretch of earth history, heated by any atomic decay, radioactivity, *in situ*. None of these controls can be easily evaluated, but there seems to be no reason, inherent in the evolutionary scheme, for regarding the present temperature at −2900 kilometers as much above or much below 4000°C. (*See* Table 1.)

At 4000° the nickel-iron just below the discontinuity would have to be far above its temperature of crystallization. The grounds for this judgment are worth noting. First let us try to make a reasonable estimate of the melting temperature of pure iron at the pressure concerned—an estimate to be made along the same lines as those for meteoritic silicate. The data now required and a series of assumptions as to possible values for $(V\text{-}v)$ and L in the case of pure iron are assembled in Table 14.

TABLE 14.—*Data bearing on the melting temperature of pure iron under the conditions ruling at −2900 kilometers*

	At 1 bar	At 1,350,000 bars (−2900 kilometers)			
$V-v$	0.0055	0.0055	0.00275	0.001	0.001
L	65	65	130	100	130
dT/dp	0.00367	0.00367	0.000917	0.000434	0.000334
Increase of T_o	.....	4955°	1239°	586°	451°
Melting temperature	1535°	6490°	2774°	2121°	1986°

But meteoritic iron contains some carbon, and $(V\text{-}v)$ for pure iron grows rapidly smaller as the proportion of dissolved carbon increases. This relation may be illustrated by the following table, which shows the findings of Honda and colleagues (Mellor, 1934, p. 11-14).

Content of carbon (graphite), in percentage	Change of volume at melting-point, in percentage
0.00 (pure iron)	+4.00 (expansion)
0.12	+3.54
0.22	+3.07
1.00	+1.60
1.90	−0.82 (contraction)
2.38	−1.98
2.86	−2.22
3.10	−2.74

The calculated values of $(V\text{-}v)$, L, and melting points of iron containing only 0.22 per cent of carbon by weight are given in Table 15.

It is possible that the percentage of carbon in the earth's core is higher than 0.22. Moreover, if, as already suggested, the metal at the top of the core is relatively rich in dissolved hydrogen and other volatile gas, the temperature of crystallization would be still farther

lowered below that expected in pure iron. Is it too extreme to suppose that at the 2900-kilometer level the core iron is 2000° above its temperature of crystallization?

Whether the core material should be rated as liquid or gas is a question not to be answered until more is known about the effect of the colossal pressure on the atomic constitution of the nickel-iron. Perhaps

TABLE 15.—*Data bearing on the melting point of iron with 0.22 per cent of carbon under the conditions ruling at —2900 kilometers*

	At 1 bar	At 1,350,000 bars (−2900 kilometers)				
$V-v$	0.0042	0.0042	0.0021	0.0021	0.001	0.001
L	65	65	100	150	100	150
dT/dp	0.0028	0.0028	0.00091	0.000607	0.000434	0.000289
Increase of T_o	.....	3785°	1230°	820°	585°	390°
Melting temperature	1530°	5310°	2760°	2350°	2115°	1920°

choice may ultimately fall on a state like that of a true gas; it is, in any case, not easy to see how any liquid at minimum pressure of 1,350,000 bars could be mobile enough to extinguish transverse seismic waves. Because of uncertainty on this point, the writer prefers to describe the core as "fluid".

What temperature is to be expected at the center of our earth-model? If the core were homogeneous chemically and in equilibrium, the central temperature would exceed that at —2900 kilometers by an amount fixed by the adiabatic relation. To calculate adiabatic heating of the kind, L. H. Adams and H. Jeffreys used the following equation (uniform density in the earth being assumed):

$$dT/dx = g\alpha T/C_p,$$

where T is the absolute temperature; g is 981 gals; α is the coefficient of thermal expansion of the material; and C_p is the specific heat at constant pressure. For the surface layers of the earth, with relatively low pressures, Jeffreys assumed α to be 2×10^{-5} per degree, and C_p to be 0.2 calory per gram degree or 8×10^8 ergs per gram degree. He found dT/dx to be about $3° \times 10^{-6}$ per centimeter or 0.3° per kilometer.

For the conditions of the core quite different values must be used. Experiments on the effect of high pressure on α and C_p suggest that the proper coefficient of thermal expansion is here much smaller than 2×10^{-5}, and the proper value of C_p is considerably larger than 8×10^8 ergs per gram degree.[26] Thus the combined effects of pressure and temperature

[26] *See* Birch (1941) where references to the original papers describing the experiments will be found. Consult also *The physics of high pressure*, by P. W. Bridgman (1931, p. 175).

make dT/dx at -2900 kilometers only a small fraction of its value at the earth's surface. Considering all changes of rate together, it does not seem entirely foolish to think that adiabatic compression has raised the temperature of the core iron by no more than 500°.

RADIOACTIVITY

Approximate temperatures for the proposed earth-model have been deduced on the premise that most of the determining heat is inherited from primitive gas, endowed with all its thermal potentialities. That premise would be false if the terrestrial core and mantle were throughout as radioactive as average iron meteorite and average stony meteorite, respectively, appear to be, and if the earth had already attained thermal equilibrium. Under such conditions the core and deeper part of the mantle would have to be much hotter than as suggested by Table 1. The earth as a whole would have grown steadily hotter. The result would be a stretching of the crust during geological time, and the very stability of the crust would be threatened. However, the geologist finds evidence of dominating horizonal compression of the crust, rather than dominating extension, and also evidence that the crust has had continuous existence since an early pre-Cambrian epoch.

In order to explain the geological facts, geophysicists have been driven to the conclusion that radioactivity is largely concentrated in a thin, superficial layer, including the sialic and basaltic rock of the continental sectors and the simatic rock of the oceanic sectors. In these crustal layers the radioactivity is supposed to be operating at the rates measured in the case of average granite, average "intermediate" rock, and average basalt. Yet, with the thicknesses of these crustal layers (which cannot be very far from those given in the first column of Table 1), the output of radioactive heat at the surface would be nearly as great as the observed output, and only a small fraction of the observed output could be attributed to original temperature at depth.

Slichter (1941, p. 583 ff.), assuming various conductivities for the earth-shells, has strengthened the generally held belief that the earth's radioactivity must decrease with increasing depth at a much faster rate than that referred to in the last paragraph. The argument on which this belief is founded becomes all the more convincing when one notes that Slichter in many cases assumed, for the deeper and thicker shells, thermal conductivities that are much greater than those suggested by experiments on rock materials at high temperatures and pressures. Most of these experimental results are assembled in the *Handbook of physical constants,* edited by Professor Francis Birch (1941) (*see* Table 16 for

the effect of temperature); others, secured by Dr. Harry Clark, technical assistant to Professor Birch, have not yet been published.

From Table 16 it appears probable that at temperatures of 1500° and upward the conductivity of the stony-meteorite silicate in the crystalline state should be below that of any ordinary rock at surface temperature; also that at a temperature exceeding 4000° the conductivity of the core iron is probably only a few times greater.

TABLE 16.—*Illustrating the relation between temperature and conductivity*

Temperature °C.	Conductivity in cals./cm./deg./sec. x 10^3							
	Mean for 3 granites	An obsidian	Mean for 3 crystallized diabases	An artificial diabase glass	Mean for 2 gabbros	Mean for 2 bronzites	A dunite	Pure iron
0	6.95	3.20	5.30	2.74	5.14	11.05	12.4	173
100	6.17	3.49	5.15	3.01	5.00	8.90	9.4	163
200	5.71	3.75	5.14	3.27	4.95	8.25	8.1	146
300	.....	3.99	5.01*	3.54	4.78**	7.3**	.....	.....
400	.....	4.25	.....	.....	4.81**	.....	.....	116
500	.....	4.52	.....	.....	.....	.....	.....	.....
800	.....	.....	.....	.....	.....	.....	.....	71

* Two specimens.
** One specimen.

In the case of nearly holocrystalline basalt at low temperature Bridgman (1924, p. 89) found the conductivity to be increased by only 6 per cent when the pressure on the specimen was raised from 1 to 12,000 bars, and he considers the relation here to be nearly, if not quite, linear. For a number of other plutonic rocks Clark (personal communication) reports a similarly moderate effect of all-sided pressure on conductivity.

Assuming the pressure-conductivity curve to be rectilinear, extrapolation gives us the following rough estimates of conductivities in the mantle of the preferred earth-model:

Depth (km)	Shell	Approximate range in conductivity	Mean conductivity
0 to −70	Lithosphere	7×10^{-3} to 4.5×10^{-3}	5×10^{-3}
−70 to −413	Asthenosphere (vitreous)	3×10^{-3} to 5×10^{-3}	4×10^{-3}
−413 to −2900	Mesospheric shell	4×10^{-3} to 16×10^{-3}	8×10^{-3}

The mean conductivity of the iron core may be not far from 50×10^{-3}.

For depths greater than 200 kilometers in most of his earth-models, Slichter assumed conductivities very much higher than those listed in the foregoing table. His computations seem to prove, therefore, that the concentration of radioactivity near the earth's surface must be still more extreme in our preferred model than in any of his models except, perhaps, his case (2), with exponential decrease of radioactivity with depth.

A conceivable cause for the upward concentration of uranium and thorium (with derived radium and its products) in a "meteoritic" earth is suggested by V. M. Goldschmidt's hypothesis that, during fractional crystallization, the relatively large uranium and thorium atoms fail to enter the space-lattices of the early-formed crystals and are left in the residual liquid. If, according to an earlier argument in the present paper, the residual liquid resulting from pressure-crystallization of the original silicate solution was impelled upward by gravity, there should have been a corresponding and important segregation of the radioactive atoms near the earth's surface.

It must be frankly confessed that such concentration of furnace heat at and near the surface to the extent apparently demanded by the preferred model does not seem probable. Perhaps just here is the most formidable difficulty in the way of acceptance of the model. However, this unsatisfactory conclusion from the brief discussion of a supremely difficult problem might have to be changed if physicists should, some day, prove that the breakup of uranium and thorium is, after all, inhibited by the conditions of the earth's interior. On the other hand, can they yet be sure that the observed radioactivity of rocks is not due to some influence outside the earth—something analogous to the impact of the cosmic ray?

DISTRIBUTION OF STRENGTH

From the morass of uncertainties about atomic decay at depth we move to somewhat firmer ground and a question no less vital—namely, the strength of the earth-shells. The relevant facts of field and laboratory have recently been assembled and interpreted in the light of two assumptions, which, however probable, are still without full proof by direct experiment (Daly, 1940). The first of these premises is that the asthenospheric glass at and above the temperature of 1200°C. lacks "practical" strength; the second, that, in spite of white heat, the material of the earth's mantle below the depth of 480 kilometers has finite "practical" strength.

Principal conclusions from that investigation are as follows: (1) Because the lithosphere is crystalline, it is strong; the reality of its strength is illustrated by the existence of mountainous relief, continued through tens of millions of years. (2) Isostatic adjustment is accomplished by flow in a layer immediately underlying the thinner lithosphere, which is a true crust. The proved approach to ultimate completeness of the adjustment for extensive loads on the lithosphere means that the flow goes on even when the displacing stress is exceedingly small. Another reason for postulating almost or quite perfect weakness for the second layer is the clear evidence of horizontal movement of large areas of the continental crust by forces which must have been minute when described in terms of the associated shearing-stress in the horizontal plane. According to the first of the two premises above mentioned, the weak layer is thought to be both vitreous and hot. From the measured surface-gradient of temperature and from the facts known about the igneous rocks, the temperature at the top of the thoroughly weak layer was estimated at about 1200°. To designate this layer it is called the "asthenospheric shell", though Barrell, who invented the term "asthenosphere", assigned to it a low degree of strength. (3) From the approximate triaxiality or ovality of the earth's figure, a strength comparable with that of the lithosphere was deduced for the likewise crystalline "mesospheric shell". The top of the mesospheric shell was located at the zone of the 20-degree discontinuity between −413 and −480 kilometers; the bottom being at the 2900-kilometer level or the top of the iron core. (4) Tentatively accepting the opinion of those seismologists who regard the earth's core as fluid, it was concluded that the core is devoid of strength.

Comparing the results with those summarized in Table 1, it is seen that the evolutionary scheme proposed in the present paper leads to an earth-model essentially like that deduced from the observations of geodesist, geologist, seismologist, and experimental physicist, all of whom have not made any necessary assumption as to a meteoritic composition for the planet.

VISCOSITY, PLASTICITY, AND RIGIDITY

Study of the deep-focus earthquakes has shown that the asthenosphere can store up enough stress-difference to induce sudden fracture (faulting) and strong shock in the weak shell. Such accumulation of stress does not necessarily imply true strength. As an alternative it would imply great absolute viscosity. In order to explain the reactions of the earth to weak cosmical forces, Schweydar postulated a viscosity of at

least 10^{12} poises, if the asthenosphere is about as thick as in our earth-model. Various writers have used the isostatic recovery (upwarping) of Fennoscandia after the removal of its last ice cap to calculate the viscosity of the slowly flowing, subcrustal material. Whether the thickness of this weak layer was taken at 2900 kilometers, that of the whole mantle of the earth, or only 100 kilometers, the viscosity was found to be enormous—the estimates running from about 10^{22} to about 10^{20}. (*See* Schweydar, 1912; 1919; Haskell, 1935, p. 265; 1936, p. 56; Vening Meinesz, 1937, p. 654; Van Bemmelen and Berlage, 1934, p. 43; Gutenberg, 1939, p. 378.)

All the estimates were made on the assumption that the isostatic flow, to correct for the negative load of late-Glacial and post-Glacial time, was not slowed down by the specially low degree of plasticity possessed by the lithosphere. Here there is room for doubt. If the earth had been cooling throughout pre-Glacial time, the lithosphere outside the glaciated tract was subject to horizontal compression, which could not have been essentially diminished by the stretching due to flow of asthenospheric material outward, in all directions from the glaciated sector of the globe. The lithosphere outside this tract may be regarded as a spherical shell, and on the principle of the dome as endowed with colossal "viscosity" which gives an approximation to "strength". With deglaciation the return of asthenospheric material to the glaciated sector must have been greatly delayed because of the slowness with which the world-circling "dome" could by "creep" within the lithosphere put its full operative weight on the asthenosphere. Until this collapse of the "domed" shell took place, the pressure-head causing flow in the asthenosphere would be small, and the rate of subcrustal flow into the glaciated sector would be small. Hence it looks as if allowance for the specially low plasticity of the lithosphere should lead to an estimate of asthenospheric viscosity which is many orders of magnitude below even 10^{20}.

However, even if that viscosity be as low as 10^{12}, serious doubt that the asthenosphere is in the glassy state may be felt. Here we have a problem almost as difficult as that of the radioactive furnace. Can our earth-model be retained when due account is taken of the low viscosity of the assumed meteoritic silicate at and just above the liquidus and at atmospheric pressure? No experiment has given a value for this viscosity, but from the figures for diopside, calcium silicate, and anorthite, it is probable that it should turn out to be well under 10^2, which is less than one tenth of the viscosity of (the more feldspathic) olivine basalt at the liquidus (Bowen, 1935, p. 249; Nichols, 1939, p. 290).

Can the pressure on the vitreous asthenosphere (which seems best regarded as not subcooled) be raised by pressure to the extent demanded?

On account of experimental difficulties no determination of the effect of all-sided pressure on the viscosity of a molten silicate has ever been made. Here we can reason only from analogy.

The most extensive experiments on the relation are Bridgman's tests on 43 liquids, with maximum pressures ranging from 1000 bars to 12,000 bars and at temperatures of 30° and 75° C. (two at 80°) (Bridgman, 1926, p. 57; 1931, p. 340 ff.). His table of results gives

"the logarithm of the relative viscosity as a function of pressure and temperature, the viscosity at 30° and atmospheric pressure being taken as unity. The logarithm of the viscosity, instead of the viscosity itself, is given because the variation with pressure of the viscosity is very rapid, and the curve of viscosity against pressure has rapidly varying curvature, whereas the curve of log (viscosity) against pressure approaches a straight line at high pressure, and is not too much curved at low pressures."

Generalizing, Bridgman (1931, p. 348) wrote:

"If the logarithm of viscosity is plotted against pressure, the curve obtained is in general concave toward the pressure axis. The curvature is much the greatest at low pressures; above 2000 or 3000 kg./cm^2 (bars) the curve approximates to a straight line in a little more than half the cases, while in the remaining cases it gently reverses curvature. This means that above 3000 viscosity either increases geometrically or else even more rapidly as pressure increases arithmetically. Among the substances investigated eugenol and p-cymene have the most rapid increase."

At 30° the viscosities of eugenol at 1000 bars and 3000 bars are respectively 3.5 and 188 times that at atmospheric pressure. At 75° the multipliers for 1000, 4000, and 8000 bars are respectively 2.4, 23.8, and 82. At 30° the logarithm-pressure curve is decidedly *convex* to the pressure axis, all the way to the maximum pressure employed—here 3000 bars. At 75° the curve is practically rectilinear out to 8000 bars, the limit of this experiment. The 1-bar viscosity of eugenol at 30° is about 50 poises. The 1-bar viscosities of 40 of the other liquids (where stated) range from 0.00198 to 0.0319, and in all of these 40 cases the logarithm-pressure curves are rather strongly concave to the pressure axis. This contrast between eugenol and the many other, much more mobile liquids suggests that the rate of increase of the logarithm with rising pressure grows with increasing value of initial viscosity.

That suggestion is supported by the recent study of Dane and Birch (1938, p. 669) on boric anhydride (B_2O_3), molten at 516° and 359°. At these temperatures the respective initial viscosities (η_0) were about 10^4 and 10^8 poises. At the melting point, about 450°, the viscosity is close to 10^5 poises (Birch and Bancroft, 1942, p. 478). The effect of pressure is well represented by the equation $\eta = \eta_0 e^{\alpha p}$, where η is the

viscosity at a given pressure—p is the pressure in bars, and a is 0.00046 cm^2/kg at 516° and 0.0015 cm^2/kg at 359°. The equation is valid up to 2000 bars. If it were valid up to 20,000 bars, the same formula and values for a would give at 516° a viscosity of 10^8, and at 359° a viscosity of 10^{21}. However, Birch and Bancroft have made it clear that "true" or Newtonian viscosity disappears when the material concerned attains rigidity of high value. This change occurs when the viscosity is no higher than 10^{10}. At all higher "viscosities" the formula given above cannot apply. Birch and Bancroft (1942, p. 479, 490) have also shown that Maxwell's theory of viscosity, involving relaxation of internal strain due to great initial rigidity, is seriously incorrect; this means that "elastico-viscosity" needs a new and very different explanation from that given by Maxwell.

After establishing these fundamental conclusions, Birch and Bancroft proceed to discuss the effect of pressure on the viscosity of the "truly" viscous liquid represented by diabase glass. Because full experimental data are here lacking, theory was invoked. The authors assumed as a rough approximation that the viscosity is "a function of the specific volume alone". On this basis it was concluded that at 1330°, the temperature assigned to the top of a basaltic, vitreous asthenosphere, an increase of pressure by 17,000 bars would increase the viscosity of the glass by 10^4; the 1-bar viscosity being taken at 10^2 to 10^3, the ultimate viscosity becomes 10^6 to 10^7 poises.

It is possible that a better choice of the underlying coefficients would give a viscosity approaching 10^8, but it looks as if the rigidity needed to account for the transmission of seismic shear waves could not be produced in basaltic glass by the pressure at −70 kilometers, namely 21,000 bars. This is one of the reasons why the writer now doubts that at the present geological epoch a vitreous basaltic layer can be world-circling unless it is only a few kilometers in thickness.

The preferred hypothesis, that the vitreous asthenosphere is almost wholly composed of ultra-basic material—peridotite passing downward into meteoritic silicate of the achondritic type—cannot be similarly treated. For this sufficient experimental data are lacking. Until the effect of pressure on the hot glass becomes known, it is impossible to tell at what pressure the relevant equation breaks down and high rigidity is first induced by rising pressure.

Meantime it is worth while to consider Bridgman's explanation of the drastic influence of high pressure on viscosity. He suggests (1931, p. 354) that there is

"an interlocking effect between the molecules, which prevents the free motion of one layer of molecules over another. Slipping of two interlocking molecules past

each other can take place only when haphazard temperature agitation has so far separated them that the interlocking parts are free. . . . When the volume is decreased at constant temperature by increasing pressure, a comparatively small increase of total volume may evidently produce a very large increase of interlocking, and the effect would be expected to increase more and more rapidly as the pressure increases. Furthermore, it is evident that the magnitude of the effect may be very different for different substances.

"Such an interlocking effect would be expected to be most important in the most complicated molecules, and strong evidence in favour of such a picture is the very marked tendency found experimentally for the pressure effect to increase as the molecule becomes more complicated. Evidently 'complication of the molecule' is a very hazy concept, and any numerical measure of it can be only very crude. It is evident, I think, that the molecule may be more complicated either because it contains more atoms or because the atoms themselves are more complicated. As a measure of the complication of the atom, I have taken the number of extranuclear electrons (the atomic number), and to measure the complication of the molecule I have multiplied the total number of extranuclear electrons in all the atoms which the molecule contains by the total number of atoms in the molecule. . . .

"If the complexity of the molecule, measured in this way, is plotted against the pressure effect on viscosity, a very definite correlation will be found."

Bridgman finds a considerable degree of similarity between his hypothesis and that of Andrade, who has suggested that "viscosity involves a temporary freezing together of the molecules of the liquid into larger aggregates more nearly crystalline in character" (Bridgman, 1931, p. 354-356; 1926, p. 57-99; 1927, p. 206; Dane and Birch, 1938, p. 669; Andrade, 1930, p. 382, 582; 1934, p. 497, 698).

To compute the average complexity of the molecules in the meteoritic silicate, we follow Bridgman's directions and then assume the material to be a mixture of 50 per cent of pyroxene, 40 per cent of olivine, and 10 per cent of anorthite. This composition cannot be exact, but it will serve to give the order of magnitude. On this basis the average complexity of the molecule was found to be about four times that of boric anhydride.[27] Bridgman's hypothesis implies, therefore, that the logarithm of the viscosity of the silicate increases, with rising pressure, much faster than the logarithm of the viscosity of the borate.

A final opinion on the subject must of course be tentative and await the results of (obviously very difficult) experimentation with silicates, but the cited analogies, together with the theoretical possibilities in the case of hot glass, seem to encourage belief: (1) that the viscosity of a vitreous asthenosphere which chemically approximates meteoritic

[27] The atomic numbers of B, Mg, Si, Fe, O, Ca, and Al are respectively 5, 12, 14, 26, 8, 20, and 13. The pyroxene was supposed to contain $FeO.SiO_2$ to the extent of one third of its weight; the olivine to contain $2FeO.SiO_2$ in the same proportion.

It should be noted that Bridgman was working with pure and apparently "true" liquids. A theory applicable to these may have to be essentially modified when applied to glass, even at high temperature. Professor Francis Birch has indicated to the writer the difficulty of determining the ultimate structure of this material. Here physicists are not of one mind, and it is possible that the atoms of molten silicate are "chained" together more effectively than are the atoms of the borate. Hence it seems unsafe to deduce from the borate experiment the quantitative effect of pressure on the viscosity of molten silicate. Direct experiment is needed.

silicate is sufficient to account for the slowness of isostatic adjustment; and (2) that at the ruling pressures—from 21,000 to 135,000 bars—the asthenospheric glass can have the high rigidity demonstrated by the seismologists for these depths.

Of less immediate importance for the geologist is the viscosity of the iron core. Because longitudinal seismic waves are transmitted through the core, and therewith are only partially damped, Jeffreys (1939, p. 266) has deduced a core viscosity at no more than 2×10^9 poises.

CONCLUSION

This paper once more illustrates a gruesome truth: In the considered words of a leading physicist, the problem of the earth's interior is "fantastically difficult". Yet in that same interior the geologist who works toward an understanding of the rocks and structures under his hammer must grope about. Clues to the properties and potencies of the mysterious depths are comparatively few, so that extrapolation from experiences at the surface have to be violent, bringing shudders to the rigorous soul of the physicist.

With some encouragement from astronomer, geophysicist, and geochemist, the geologist can readily think of a "meteoritic" earth, but for good reasons those colleagues have not decided on any detailed story of evolution for such a planet. What is to be done? Leave the deep interior "to the poets" as one distinguished geologist recommended? Or accept the general idea of a "meteoritic" earth, develop it with some degree of reasonableness, and then return the hypothesis of an evolved "meteoritic" earth to masters in the three related sciences for criticism? Choice of the second alternative was the motive for the present paper. Will those masters find the suggested earth-model of value in guiding research on the properties of terrestrial materials at high pressures and temperatures? How can the model be made more plausible by modification, by taking better account of experimental and other observed data already in hand?

With full and painful consciousness of inadequate training for the job, the writer has tried to test the preferred model. The criteria include comparisons between the deduced planet and the actual planet, with respect to its (geoidal) shape, its mean density, its moment of inertia, its radioactivity, and its discontinuities, as well as the density, composition, state, elasticity, strength, and plasticity of the constituent shells and core.

And there is another side to the argument, a second chain of deductions, this time concerning the origin of the meteorites themselves. The ruling

idea of a parental planet for these, and the hypothesis that it, like the earth, is descended from a separate mass of stellar gas, are adopted as major premises. In earth and parental planet alike, two-phase convection and associated pressure-freezing seem to be logical consequences of the mode of evolution, from gas through liquid to crystalline solid. Two-phase convection and associated pressure-freezing in their turn give a new and much-needed explanation of the fine grain and confused crystallization of the stony meteorites, thereby tending to support the evolutionary hypothesis so far as it is the same for both earth and parental planet. Because dikelike tongues of nickel-iron cut through the silicates of meteoritic stones, as if the free metal was liquid when the silicate was already crystallized, one is helped toward agreement with those seismologists who think the earth's core to be "fluid" while at least the greater part of the mantle is crystallized. Finally, the composition of the meteorites is just that expected if the parental planet, like the earth, were differentiated by gravity during the early evolutionary stages. Thus, the deductions about the constitution of the earth, an intact planet, appear to be strengthened by a fair appraisal of meteoritic stones and irons as fragments of a disrupted sister planet.

In the present paper no attempt has been made to cover tests of a more dynamical character—those that should be passed by the suggested earth-model if, during its own evolution, it has *worked* in accordance with the facts of geology. Examples, all of portentous difficulty in application, are involved with the problems of mountain-building, continent-making, the formation of geosynclines, and other broad warpings of the earth's crust. However, it seems possible that even these distortions of the crust may also furnish criteria favorable to the idea that the earth is of "meteoritic" composition and developed according to some (though not all) of the processes assumed by Jeffreys.

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ALPINE MUDFLOWS IN GRAND TETON NATIONAL PARK, WYOMING

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ABSTRACT

The mudflows described formed in August 1941 in the Teton Range, Wyoming, and are of the little-known alpine type. Most of them occur in the larger canyons at 7500 to 8500 feet altitude, beneath talus slopes at the base of couloirs in the canyon walls. Individual mudflows vary in pattern due to differences in volume, gradient, and topography. They are distinguished by sandy matrix and a profusion of relatively small boulders. Where mudflows invaded steep sections of the canyons their lower portions were reworked by flood waters of the main streams, and consequently typical mudflows grade downward into roughly sorted deposits somewhat like torrent fans.

Factors responsible for the formation of these mudflows include (1) development of unstable talus slopes as they approached saturation following prolonged rainy and cloudy weather; and (2) exceptionally heavy and sudden rain which caused short-lived torrents to descend the steep couloirs of the canyon walls onto unstable talus beneath.

The Teton mudflows demonstrate that in alpine regions, as in semiarid, unusual weather conditions may initiate mudflows in numbers so large as locally to overshadow many other geomorphic agencies. They then play a significant role in moving rock debris out into the zones of stream attack, in the central parts of the canyons. Here they ultimately lose their characteristic mudflow form as the fine matrix is swept away and the boulders are distributed downstream.

INTRODUCTION

A summer of record precipitation in west-central Wyoming was climaxed in Grand Teton National Park in August 1941 by the formation of approximately 100 mudflows, a phenomenon not previously reported from this region. Several major canyons were profoundly changed by scarring of canyon slopes and deposition on canyon floors, where stream courses were shifted or obstructed.

ACKNOWLEDGMENTS

This paper is part of a larger study of the Teton Range which in the summer of 1941 was supported by a grant from the Penrose Bequest of The Geological Society of America. The authors acknowledge their indebtedness to this source and also their obligation to officials of the National Park Service who assisted in many ways. Chief Ranger Allyn Hanks compiled meteorological data used in this study, and Superintendent Charles J. Smith and Naturalist Carl Jepson contributed valuable observations. Robert Crist and Joseph Hoare were very capable assistants. Thanks are particularly due Dr. R. P. Sharp, whose critical reading of this text proved invaluable.

WEATHER CONDITIONS BEARING ON MUDFLOW FORMATION

Meterological records indicate a close relationship between the unusual weather and the formation of mudflows. There are no data for the uninhabited alpine zone within which the mudflows formed (altitude 7500 to 11,500 feet), but records are available for near-by co-operative weather stations outside the mountains. The closest station is that which has been maintained since 1936 by the National Park Service at the Headquarters of Grand Teton National Park, situated at Moose (altitude 6627 feet) about 3 miles east of the nearest mudflows (Fig. 1).[1] This station has furnished pertinent data which are summarized in the tables.

[1] The next closest station is that maintained by the Forestry Service at Moran, 8 miles east of the nearest mudflows (Fig. 1). Its record closely parallels that of the Headquarters station herein summarized.

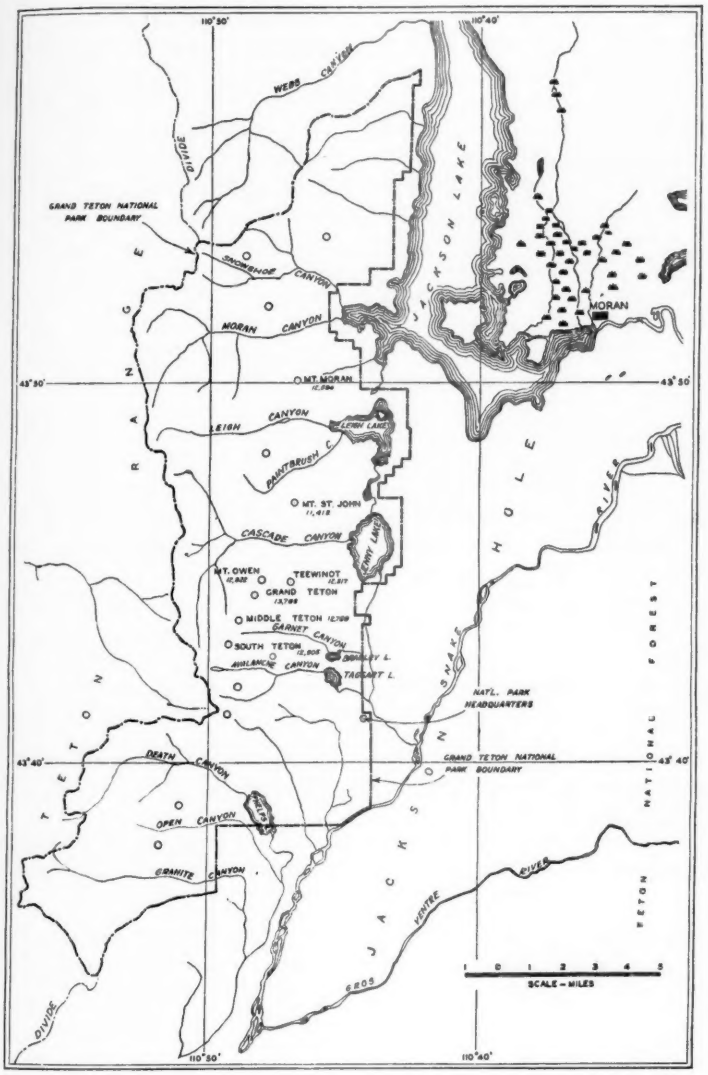

FIGURE 1.—*Index map*

TABLE 1.—*Cloudiness and rainfall record*

		June	July	August	Total for 3 months period
Cloudiness	Days clear:				
	Average number, 1936–1940..	9.4	10.6	15.8	35.8
	Number in 1941*.....	3	4	2	9
	Days partly cloudy:				
	Average number, 1936–1940..	11.8	12.8	10.6	35.2
	Number in 1941.....	11	14	15	40
	Days cloudy:				
	Average number, 1936–1940..	8.8	7.6	4.6	21.0
	Number in 1941.....	16	13	14	43
Rainfall	Days with measurable rainfall (.01 inch or more):				
	Average number, 1936–1940..	11.6	9.8	7.2	28.6
	Number in 1941†.....	17	16	20	53
	Days with .10 inch or more rainfall:				
	Average number, 1936–1940..	5.4	4.6	3.6	13.6
	Number in 1941.....	7	7	11	25
	Rainfall totals:				
	Average rainfall, 1936–1940..	1.83 in.	1.20 in.	.79 in.	3.82 in.
	1941 rainfall.....	2.24	1.70	3.83	7.77
	1941 departures from the average.....	+.41	+.50	+3.04	+3.95

* In June, July, and August 1941 only once did two clear days occur in succession.

† During this 3 months at no time did more than three successive days elapse without precipitation.

Table 2 gives the rainfall distribution for the 25 days in the 3 months when precipitation at Headquarters amounted to .10 inch or more, and the rainfall for each day.

No specific data are available from the Tetons on altitudinal variation of rainfall, but it is believed that the zone of maximum pre-

TABLE 2.—*Rainfall distribution*

June		July		August	
4	.10 in.	7	.17 in.	5	.33 in.
5	.12	8	.17	8	.13
7	.30	14	.27	9	1.04
8	.18	19	.17	11	.29
9	.29	20	.18	12	.60
20	.45	21	.11	15	.24
28	.25	25	.25	16	.10
				17	.10
				19	.35
				24	.15
				27	.11

cipitation lies above the base of the range. If so, in the zone of mudflow formation there were probably more than 53 rainy days in the 3 months, and rainfall totals probably exceeded those of Headquarters, at 6627 feet on the floor of Jackson Hole a mile east of the mountains. The writers experienced several rains in the range, some of cloudburst proportions, not reflected in the Headquarters record. The cloudiness of the summer as indicated at Headquarters is significant because "partly cloudy" or "cloudy" in Jackson Hole almost invariably means rainfall somewhere in the adjacent Tetons.

On the southwest slope of the Wasatch Mountains of Utah, A. J. Henry (1919, p. 40) found that in both 1914 and 1915 the precipitation from June to September inclusive increased with altitude between 8000 and 10,000 feet at the rate of 1 inch for each 1000 feet. A similar ratio of increase, if applied to the Tetons, gives a rainfall total for June, July, and August 1941 of about 11 inches at 10,000 feet, as compared with the 7.77 inches which fell at Headquarters. This figure is only suggestive. It may be too low for the Teton east slope, where the presence of the rank of high peaks possibly makes for a higher ratio of increase.

The frequency of rains in the summer of 1941 and their unusually great volume (for the season) resulted in rainfall totals that at Headquarters, outside the Teton Range, were above average throughout the summer and higher for each succeeding month; in August rainfall was almost five times the average. At higher altitudes, where the mudflows formed, conditions may have been even more exceptional. Continued cloudiness inhibited the drying off of mountainsides, and by late summer many detrital deposits in the range were apparently almost saturated. Continued rains in early August were climaxed on the night of August 9-10 when almost twice as much rain fell as at any other time in the summer. This provided the "trigger effect" necessary for initiation of mudflows, which simultaneously made their appearance throughout the alpine region of the range.

CIRCUMSTANCES OF MUDFLOW FORMATION

Superintendent Charles J. Smith of Grand Teton National Park states that on the evening of August 8 heavy rain and flood conditions drove his trail crew from its camp near the forks of Cascade Canyon (Fig. 1) to a near-by refuge. "Heavy rumbling noises" reported by John Ray, foreman of this crew, were possibly due to avalanches in Cascade Canyon, and he believes that the formation of mudflows started at this time.

On the following evening, according to Naturalist Carl Jepson, a park visitor descending Cascade Canyon after dark through a downpour of rain likewise encountered flooding creeks and heard roars which she

attributed to avalanches. At two points she found the trail obstructed by fresh mudflows not present in the morning.

Widespread formation of mudflows probably took place later on the night of August 9-10, when torrential rains fell which mountaineers camping at timberline described as "terrific cloudbursts." The next morning many fresh mudflows were reported, some of which obstructed the Cascade Canyon trail. Paul Petzoldt, guide on the Grand Teton, observed a mudflow that had spread out on Middle Teton Glacier at about 11,500 feet. Robert Crist and Joseph Hoare found "streams of rock debris" at 9500 feet on Teewinot still so fresh that when stepped on the saturated mixtures resumed flow "with a churning motion and grinding sound." On August 11 the writers observed from a distance these Teewinot mudflows and similar ones on Mount St. John and other peaks. On August 14 we discovered small mudflows, apparently several days old, in Webb Canyon; on August 25, the much larger mudflows in Leigh Canyon. Though a few of the Leigh Canyon mudflows were still soft in places all the mudflow activity in this canyon probably occurred on August 9-10. The same conclusion was reached for other mudflows later found in scattered parts of the range.

It is therefore believed that the great majority of the Teton mudflows originated on the night of August 9-10. Possibly a few formed the night previous, and during the week of August 11-19, when other heavy rains fell. It is not clear whether any such subsequent activity involved renewed movement of previously formed mudflows, further contributions to them, or the launching of new mudflows.

DISTRIBUTION OF THE MUDFLOWS

The mudflows formed throughout the alpine portion of the Teton Range, which corresponds closely to the area of Grand Teton National Park (Fig. 1). The Park is an irregular strip of the Teton east slope that is 9 miles in maximum width and 27 miles north-south. Mudflows were found outside the park only in Webb Canyon, a mile or two beyond the north boundary.

Within this rugged tract of nearly 200 square miles, mudflows originated along the walls of every canyon and on the upper slopes of most of the peaks. Their altitudinal range was from about 7500 feet, near the foot of the range and along the base of the canyon walls, to 11,500 feet on the peak slopes. The larger mudflows occur at 7500 to 8500 feet.

Most striking development of mudflows took place in the vicinity of Mount Moran, in the north end of the range, and in Cascade Canyon in the central portion. The lower slopes of Mount Moran were streaked

by mudflows that descended radially into Moran and Leigh canyons and the cirques to the east. Thus in Moran Canyon the mudflows occur chiefly along the south wall; in Leigh Canyon, along the north wall. In Cascade Canyon the majority of the mudflows came off the south and

FIGURE 2.—*Generalized sketch of a Teton mudflow*
Showing its relation to talus and couloir above.

southwest faces of the rugged Mount St. John group of peaks and hence occur along the north wall of the canyon.

DESCRIPTION AND INTERPRETATION OF THE MUDFLOWS

UPPER LEIGH CANYON

Description.—The mudflows of Leigh Canyon (Fig. 1) merit detailed consideration, since here they formed on an impressive scale and under conditions especially favorable for observation.

Upper Leigh Canyon is fairly broad and open, and of low gradient, the descent of the canyon floor being from 9500 to 7750 feet in 4¼ miles. This portion of the canyon gave no indication of the destructive flood waters for which much evidence was found in lower Leigh Canyon. The mudflows therefore were left unmodified. Furthermore, in this section many mudflows were so widely spaced as to permit individual deposits to retain separate identity.

The mudflows here all have much the same general relationships. They occur at the base of talus slopes and, with a few exceptions, are confined to the north wall of the canyon. Each mudflow can be traced upslope to the mouth of a gully, or gully complex, extending on up the fan or cone of talus to its apex, which in turn is situated at or just below a precipitous couloir in the canyon wall (Fig. 2; Pl. 1, fig. 1; Pl. 2, fig. 1). The gullies, some of which branch in complicated manner, are as much as 25 feet deep. They are typically flat-bottomed, boulder-floored, and in many instances bordered by mudflow levees.[2] Where the gullies cut through the forest cover on old talus they are conspicuous raw gashes (Pl. 2, fig. 1).

Correspondence between the size of the mudflows and the associated talus deposits was observed at least to the extent that the largest mudflows formed beneath the largest talus slopes. Talus of gentle slope as well as steep gave rise to mudflows, there being little uniformity in this respect.

Individual mudflows vary widely in pattern, reflecting differences in their volume and gradient, as well as control by underlying topography. The smallest ones—tens of feet wide—are simple streams of debris, uniform in breadth or widening somewhat downward, that terminate as blunt toes thrust into the forest of the lower canyon slopes (Pl. 2, figs. 1, 2). Many other mudflows are complex and anastamosing. On the lower slopes the mudflows fan broadly or become lobate. Large ones push out onto or across the canyon floor, in some instances giving off lobes that extend down or up the axis of the canyon, or both (Fig. 2; Pl. 2, fig. 1). In the invaded tracts, scrub growth has been wiped out, small trees being bent sharply downslope, uprooted, or snapped, and large trees surrounded and much scarred at the level of the mudflow surface.

In several places mudflows advancing from the north wall forced Leigh Creek into a new channel along the south wall. Here its course now loops around the mudflow toes (Fig. 2; Pl. 2, fig. 1). Where the creek is more seriously obstructed, ponds and marshes have formed, inundating vegetation on the canyon floor.

Surface gradients of several large mudflows vary from 7 to 15 degrees on their steep apical portions and are 3 degrees or less at their expanded lower margins. Along their sides and ends the mudflows terminate in abrupt and more or less steep fronts, commonly 2 to 6 feet high but exceptionally 10 or 12 feet high.

The expanded lower parts of most mudflows are broadly convex upward in cross section and are without channels, ridges, or other such irregularities (Pl. 2, fig. 2). In the narrow, steep upper parts the surface is uneven and exhibits longitudinal furrowing. In these upper

[2] Term proposed by R. P. Sharp, unpublished manuscript.

FIGURE 1. LARGE MUDFLOW IN LEIGH CANYON
Note gully extending down talus slope to mudflow.

FIGURE 2. LAKE PRODUCED BY LARGE MUDFLOW OF FIGURE 1.

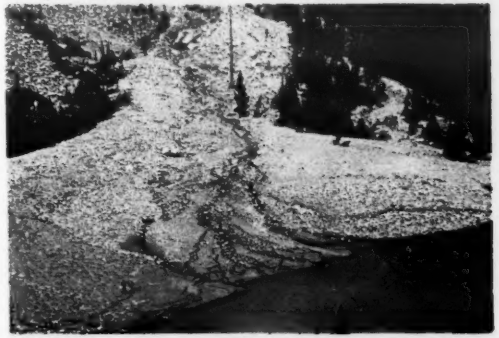

FIGURE 3. CENTRAL PORTION OF MUDFLOW OF FIGURE 1.
Note man in lower left for scale.

FIGURE 4. SURFACE DETAIL OF MUDFLOW OF FIGURE 1

LARGE MUDFLOW IN LEIGH CANYON

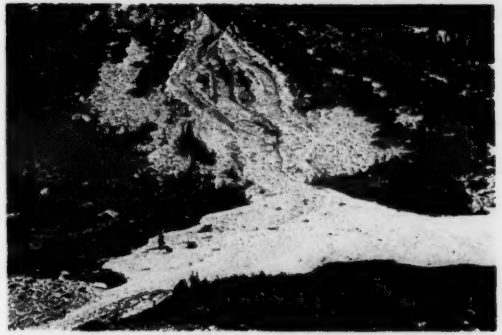

FIGURE 1. MUDFLOW IN UPPER LEIGH CANYON
Showing lobes on canyon floor and relation to gullies and mudflow levees in talus slope above.

FIGURE 2. LOBATE FRONT OF MUDFLOW
Deployed on floor of Leigh Canyon.

FIGURE 3. ABRUPT FRONT OF MUDFLOW IN CASCADE CANYON
Large boulders were carried to edge of deposit.

FIGURE 4. CONVERGING MUDFLOWS FROM NORTH AND SOUTH SLOPES OF CASCADE CANYON
They met on canyon floor and dammed Cascade Creek.

MUDFLOWS IN LEIGH AND CASCADE CANYONS

parts many mudflows have well-developed median channels, bordered by mudflow levees several feet high, that can be traced upslope to the gullies in the talus above.

The texture and composition of the mudflows are quite similar. Their most distinctive characteristic is absence of any sorting or stratification. The matrix consists of light-gray mud, silt, or sand, composed of undecayed mineral and rock particles, with quartz, feldspar, and micas as predominant mineral constituents. The matrices are gritty, ordinarily more sandy than clayey. Imbedded at random in the matrix and protruding everywhere from the mudflow surfaces are irregular rock fragments and boulders (Pl. 1, fig. 4; Pl. 2, figs 2-4). The proportion of such bouldery material to the volume of associated matrix is very high for mudflows, but no individual boulders seen were more than 4 feet in diameter. This narrow size range of the surface boulders results in a texture which, compared to the coarse and irregular adjacent talus, appears relatively fine and uniform (Pl. 1, figs. 1, 3; Pl. 2, fig. 1). Throughout their extent the individual mudflows show fairly little textural variation. Boulders are in general as abundant at the front of the flows as farther back.

The largest and in many respects the most interesting mudflow in the range occurs about 1½ miles up Leigh Canyon, where it formed at the base of the huge talus cone occupying the re-entrant just west of Mount Moran. It is a fan-shaped deposit (Pl. 1) measuring 500 feet from apex to bottom and 700 feet across the front, where two lobes formed, one extending 100 feet up the canyon, the other 150 feet down the canyon.

This mudflow completely blocks the course of Leigh Creek, giving rise to a lake about 500 feet wide and 1000 feet long (Pl. 1, fig. 2). Water backed against the south cliffs makes the canyon impassable on this side. Along the north and west sides of the lake marginal trees are partially submerged. (The forest here has been entered, and its trees surrounded by many small mudflows.) The mudflow forms a substantial barrier which will doubtless impound the lake for many years.

Practically all the forest growth in the path of this mudflow was wiped out except one isolated clump around which the mudflow divided. Outward overturning of trees indicates the radial character of the mudflow movement.

A well-defined stream channel descends this mudflow and, near its base, divides into distributaries. Inter-distributary areas are covered with mud and sand that merge with deltas at the lake front (Pl. 1, fig. 3).

When this mudflow was visited on August 25, the section crossed by distributary channels was still saturated and too soft to walk on, having much the same consistency as wet quicksand. This was also true of

some adjacent areas in the central part of the mudflow. Marginal tracts, however, were entirely firm and almost dry. A few low concentric ridges (Pl. 1, fig. 3, right-hand side of mudflow) suggested that this mudflow might be composite, the result of a small later flow superimposed on the main one. However, these ridges more probably resulted from forward motion or pulsations developed within a single flow. The softness of the central area was probably due to exceptional thickness and to the replenishment of water from the distributaries, which must have carried small streams for days in order to have developed their channels and deltas.

Interpretation—The continuity of couloirs, gullies, and mudflows bespeaks genetic relationship, suggesting a process of mudflow formation which may be visualized somewhat as follows:

The heavy rain which provided the "trigger effect," initiating the mudflows, was carried off Mount Moran and its west ridge (Fig. 1) by couloirs which thus were swept by short-lived torrential streams. Because the couloirs narrow and steepen below, these streams converged into torrents of tremendous volume and force which, directed diagonally downward upon the talus slopes beneath, acted like huge hydraulic hoses. Already unstable because near saturation, the talus was churned into a mixture of debris and water which gave way and flowed downslope. In its wake was left the flat trench out of which material had issued; lower the stream of debris came to rest as a mudflow. Some of the debris may have been swept out of the couloir, but probably most of it was derived from the talus.

Along the south wall of Leigh Canyon talus accumulations and couloirs are somewhat smaller than along the north wall, but otherwise similar. The smaller drainage areas of the couloirs and the correspondingly smaller discharge of water on this side, or the spotty distribution of rainfall, in turn related to the relative altitudes of the adjacent peaks, may account for the failure of mudflows to form here on a large scale. The altitude of the main peak south of the canyon is 10,585 feet as compared with 12,694 feet for Mount Moran, the main peak north of the canyon.

The relatively fine and uniform texture of the mudflows, as compared with the coarse and irregular talus, implies a degree of selection, reflecting size limit of material that the mudflows could pick up and transport. In the gullies above the mudflows many boulders exceed 4 feet and presumably were left behind because the mudflow could not transport them (Pl. 1, figs. 1, 3; Pl. 2, fig. 1).

Gullies in the talus slopes are continued downward in well-developed

channels, bordered by mudflow levees, which descend the steep, upper parts of the mudflow. It is suggested that at the mudflow heads, where gradients were high, such channels were cleared out by the increasingly fluid streams that characterized the closing phases of mudflow formation. Along the sides of such channels coarse debris was pushed aside in snowplow fashion to form the bordering levees.[3] Downward, channels and levees both die out where the gradient flattens and the principal accumulation of debris begins.

The steep, narrow upper portions of many small mudflows are only veneered with fresh debris, being essentially mudflow trails down which the bulk of mudflow material moved as it descended to lower levels.

Certain channels (Pl. 1, fig. 3) which run the full length of the mudflows and cross their expanded basal portions are thought to have been developed by streams which, after the flow of debris had completely stopped, continued to discharge in diminishing volume from the couloirs above.

LOWER LEIGH CANYON

Description.—Leigh Canyon (Fig. 1), like other major canyons in the range, is hanging where it opens on Jackson Hole, and its steep lower section, which drops 600 feet in 1½ miles, exhibits features differing from those in the upper canyon.

A mile west of Leigh Lake, a group of mudflows originated at the base of north-wall talus slopes, advanced southeastward into the canyon, and converged into a lobe that floored the canyon and moved downcanyon ¼ mile. Brush and scrub forest were wiped out within the area of this lobe, and trees nearly a foot in diameter were uprooted or broken off and stripped of limbs, bark, and roots. Leigh Creek now crosses this deposit in a new and difficult course through a complex of channels.

The mudflows of this section are essentially similar to those in the upper canyon, but where they merge the resultant lobe is almost exclusively made up of boulders. The boulder bed gives way along an irregular front to a freshly deposited sheet of poorly sorted gravel and sand, several hundred yards long. This divides into several winding streams of sand, 3 or 4 feet thick, which can be traced through the brush of the canyon floor into the forest west of Leigh Lake. Here are large trees freshly uprooted and thrown downstream, and newly formed log jams well above normal stream levels.

Interpretation.—It is thus evident that the mudflows of lower Leigh Canyon differ from those of the upper canyon in that their basal portions have been subjected to stream action and reworked into deposits

[3] R. P. Sharp, unpublished manuscript.

having some of the characteristics of torrent fans (Matthes, 1930, p. 108-109).

In this section of the canyon, where water in great volume was contributed from the high peaks on both sides, Leigh Creek rose to flood stage either at or soon after the time of maximum mudflow activity. This augmented volume and the locally steepened gradient enabled the creek to attack the confluent mudflows that advanced across its course. The mudflow boulders were redistributed into a boulder bed, and the finer matrix materials were carried beyond them, in part being spread out to form the sheet of gravel and sand. Still lower in the canyon, the flood waters recorded their sweep by additional sand deposits, large overturned trees, and new log jams formed above the normal levels of Leigh Creek.

MORAN CANYON

In Moran Canyon (Fig. 1) conditions parallel those in Leigh Canyon, except that mudflow features are less striking and occur chiefly along the south canyon wall. However, several fairly large mudflows and many small ones also formed along the north wall.

In the relatively flat upper canyon, most of the mudflows are distinct structures and show little or no modification by stream action. None was large enough to obstruct the drainage seriously.

In the steep lower canyon, the relationships duplicate those in lower Leigh Canyon, but on a smaller scale. Mudflows originating from talus on the lower north slopes of Mount Moran converged on the canyon floor into a lobe that advanced several hundred yards down the canyon. Moran Creek removed most of the finer materials from this lobe, leaving a train of coarse residual boulders.

CASCADE CANYON

In Cascade Canyon (Fig. 1), at least two dozen distinct mudflows were observed in the 3½-mile stretch from Jenny Lake to the forks of the canyon. Most occur along the north wall, having moved off the lower slopes of the Mount St. John group of peaks.

In their relations to talus slopes and couloirs, as well as in composition and texture, these mudflows resemble those already described. They too occupy a section where the canyon gradient is low and they are essentially unmodified by torrent action. In several places small mudflows are closely spaced, converging and diverging as a mudflow plexus.

About ¾ mile above Jenny Lake, several mudflows moved toward each other from opposite sides of the canyon, met, and coalesced on the canyon floor (Pl. 2, fig. 4). Obstruction of Cascade Creek at this point resulted in formation of a lake about 400 feet long and 200 feet wide.

Like Leigh and Moran canyons, Cascade Canyon steepens abruptly in its lower part, but no important mudflows entered this section. However, mudflows invaded the steep north fork of the canyon, and here mudflow features grade into those due to stream action on the largest scale observed. Two large and nearly parallel mudflows descended from a great talus cone on the northeast canyon wall and coalesced at stream level into a flat symmetrical lobe that pushed downcanyon almost ½ mile, flooring the canyon. The north fork trail and bridge were destroyed, and a clean path was cut through the forest.

In its relation to tributary mudflows and in its bouldery composition this great lobe duplicates the similar deposits in lower Leigh and Moran canyons. It has undoubtedly had much the same history, illustrating even more impressively the reworking of mudflow material into torrential deposits.

HIGH PEAK AREA

Features thus far considered are all confined to the major canyons at an altitude between 7500 and 8500 feet. Mudflows also formed within cirques of the mountain peaks, at altitudes of 9500 feet and above. There was no opportunity to study these in detail, but distant observation and accounts of mountaineers indicate that these mudflows differ somewhat from the others. Most of them originated within couloirs, or at their mouths, and resulted from movement of debris which had previously accumulated in the bottoms of couloirs. (All but the steepest couloirs are cluttered with such debris, mostly frost-riven from the couloir walls.) For some days after August 10, mountaineers found the loose rock in the couloirs so saturated and unstable as to make usual climbing routes difficult or impossible.

In the open cirques below the couloirs, many of these mudflows divided into a complex of anastamosing streams which flowed down onto talus slopes. Few if any were large enough to expand much on the cirque floors.

There were probably many mudflows above 9500 feet that derived their material from talus rather than from the debris of couloirs, but none was observed. Ranger Naturalist John Reinemund (personal communication) observed some small mudflows that formed on the slopes of the fresh terminal moraine bordering Teton Glacier (on the northeast side of the Grand Teton, Fig. 1).

DAMAGE BY MUDFLOWS

In terms of timber and scenic values, damage to the forests of Grand Teton National Park attributable to the mudflows of 1941 was considerable, though difficult to estimate. Within the park there are no

buildings except abandoned log cabins, and the few miles of road lie outside the range. Hence only trails and bridges were affected, and there were none in Leigh or Moran canyons. A well-defined game trail up Moran Canyon was wiped out over much of its course. A modern trail system extends through the central and southern parts of the park, from Paintbrush to Granite canyons (Fig. 1). Concerning damage here, Superintendent Charles J. Smith (personal communication) states:

"At least one-half mile of trail was lost by the action of the mudflows, including several bridges, and many other sections of the trail were damaged. Approximately $1000 was the cost of fair repair to the trails and bridges and more will be needed to put them in as good condition as before."

DISCUSSION OF MUDFLOWS

In general the Teton mudflows fit well into the category of mudflows as defined by Sharpe (1938, p. 55-61). The conditions most favorable to the occurrence of mudflows are (Sharpe, 1938, p. 56); ". . . (1) abundant but intermittent water supply, (2) absence of any substantial cover of vegetation, (3) unconsolidated or deeply weathered material containing enough clay or silt to aid in lubrication of the mass, and (4) moderately steep slopes." To these factors we may add two others which were critical in the Teton mudflows: (1) development of instability in talus slopes as these approached saturation following prolonged rainy and cloudy weather; and (2) occurrence of an exceptionally heavy and sudden rain which caused short-lived torrents to descend the steep couloirs of the canyon walls onto unstable talus beneath.

Of the three rather well-defined types of mudflow (Sharpe, 1938, p. 57)—the semiarid, alpine, and volcanic—the Teton mudflows most closely approximate the least known alpine type. Distinguished most obviously by their high-altitude occurrence, alpine mudflows may form where the annual precipitation considerably exceeds the 10 to 20 inches usually implied by the term semiarid. In the Tetons, the average annual precipitation at Headquarters from 1936 to 1941 inclusive was 27.07 inches; and, as discussed under Weather Conditions Bearing on Mudflow Formation, precipitation may have been considerably greater in the alpine zone where the mudflows formed.

Sparsity of vegetation, characteristic of mudflows of all types, seems to have had limited importance in the Tetons, where a number of mudflows formed from talus slopes largely overgrown with vegetation. Penetrating roots must help stabilize talus slopes, and the fact that mudflows formed in spite of forest cover suggests the exceptional nature of the conditions which locally initiated flowage.

In certain respects the Teton mudflows involve features not described or emphasized in the literature and which may be characteristic

of the alpine type. Most significant of these is the intimate association of mudflows with superjacent talus and couloirs. In alpine mudflows elsewhere, such association should prove common since at high altitudes rock debris characteristically occurs as marginal talus beneath the couloirs of canyon walls. Also, the matrix of the Teton mudflows is rather more sandy than in other described mudflows and carries bouldery material in greater profusion.

In view of the emphasis given to transportation of very large boulders by semiarid mudflows (Blackwelder, 1928, p. 470-473) the composition and texture of the Teton mudflows possess interest. The Teton mudflows were unable to pick up and transport boulders exceeding 4 feet in diameter, but the mudflows of semiarid regions carry boulders twice to three times this diameter on much lower gradients. Probably the difference is related to the more sandy makeup of the Teton mudflows. Whether this difference holds true for all alpine regions is a question which awaits further study. Some small alpine mudflows in the San Juan Mountains, Colorado (Sharpe, 1938, p. 59-60), seem to have had texture comparable to that of the Teton mudflows, since "one studied in detail had transported blocks as large as three by three by five feet to the toe of the flow, although six- to twelve-inch sizes were more common within the mass."

Desert mudflows described by Blackwelder (1928, p. 474) were mostly 6 to 20 inches thick, and the thicker ones were only 3 to 6 feet at their edges. Most Teton mudflows ranged from 2 to 6 feet thick, but a few attained marginal thicknesses of 10 to 12 feet. Blackwelder's observation that the thicker mudflows were less fluid may apply generally and suggests that the Teton mudflows were of heavy consistency. The sandier matrix and the bouldery character of these mudflows may again furnish the explanation.

The distinctive sandy and bouldery composition of the Teton mudflows is related to derivation of these mudflows from talus deposits which are the product of high-altitude weathering of gneiss and pegmatite. Obviously weathering of such rocks under other conditions might yield debris much less sandy, and with relatively fewer boulders. Thus, mudflows of different regions may exhibit variations which, to an important degree, are determined by the lithology of the unconsolidated materials involved and the weathering processes.

Ekblaw (1917), Blackwelder (1928, p. 474, 480), and others have shown what significant contributions mudflows may make to the playa and piedmont alluvial deposits of semiarid regions. A similar role for the mudflows of high altitudes is indicated by those Teton mudflows which originated in couloirs and added their bulk to lower talus slopes.

Other mudflows which formed from talus slopes and moved a short distance simply reworked and extended the detrital deposits. In these instances the alpine talus slopes, like the semiarid alluvial deposits, were augmented and complicated by the intercalation of mudflows, especially in their lower portions. However, the majority of the Teton mudflows, especially the large ones, derived their substance from talus slopes and moved out to new positions beyond the talus fronts. These mudflows actually subtracted from the volume of the talus slopes.

Blackwelder (1928, p. 469) comments that the mudflow he described from Willard, Utah,

> ". . . was a normal but rare occurrence. The conditions observed in the canyon suggest that at any one locality two such events may be separated by centuries of time, during which the soil covering, talus, and vegetation are regenerated."

In the Tetons, no mudflows had previously been noted during our observation (1925-1941), nor was there any evidence suggesting that large mudflows had formed within the several decades previous to 1925. Events of 1941 therefore confirm the view that mudflows may be "normal but rare," demonstrating that in alpine regions mudflows may, after lapse of long intervals, suddenly originate in large numbers and locally overshadow many other geomorphic agencies.

When alpine mudflows do form they play a significant role in moving rock debris from the marginal talus slopes outward into the central parts of the canyons. These are the zones of stream attack, and where the canyon streams have steep gradients they may rework the mudflows even while the latter are advancing. If not then, sooner or later the mudflows lose their characteristic form as streams and other erosional agencies remove the finer matrix materials and redistribute the boulders downstream. The ubiquitous boulder beds that floor the canyons in the Tetons and other ranges may in many instances be referable to old mudflows no longer recognizable as such.

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